BRAITHWAITE'S RETROSPECT.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D., LOND.,

ASSISTANT-SURGEON TO THE LEEDS HOSPITAL FOR WOMEN AND CHILDREN.

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Letters for the Editors, to be addressed

Dr. Wm. Braithwaite, Clarendon House, Clarendon Road, Leeds ; *or*

Dr. James Braithwaite, Clarendon Road, Leeds.

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SYNOPSIS,

(ARRANGED ALPHABETICALLY) CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—It often happens that we meet with cases of ague which have been unbenefitted by several weeks of quinine taking. In most of these cases what is wanted is relief to the engorgement of the internal organs. Two or three grains of colocynth and rhubarb in combination with quinine will often at once cure the disease. (Dr. H. W. Fuller, p. 46.)

CANCER.—That cancer is to be regarded as a blood disease requires stronger evidence than has yet been adduced. It is really a parasitic disease in its nature. To a certain extent a constitutional tendency must be admitted, just the same as there is to sebaceous tumours and warts, but no one ever thinks of calling these the results of a blood disease. (Mr. C. De Morgan, p. 27.)

Cancer flourishes best when there is plenty of good blood, and grows very slowly when the blood is poor. If a patient with cancer of the uterus is kept on the sparest vegetable diet, just enough to keep body and soul together, the disease will be arrested and may disappear, as it has been known in one well-authenticated case to do. The antagonism between tuberculosis and cancer may be explained by the fact that tuberculosis is associated with waste of tissue. (Mr. C. De Morgan, p. 38.)

GENERAL DEBILITY DEPENDING ON DEFICIENT ELIMINATION.—There are many cases of so-called debility, the symptoms being chiefly extreme languor, weakness, and total loss of appetite. They are treated with tonics, chalybeates, and good food with stimulants—but without success. The fault is our not examining the secretions with sufficient care. The tongue may be clean and the bowels said to be regular, but the urine will be scanty and loaded with lithates, and on examination the motions will prove to be white, lumpy, and offensive. The fact is that the patient is oppressed with the presence in the blood of matters which ought long since to have been

eliminated, but which the inactivity of the secreting organs has caused to be retained in the system. If a few doses of calomel and colocynth are given the urine will become abundant and pale, and the motions normal in character, and the tonics which previously did harm will now rapidly effect a cure. (Dr. H. W. Fuller, p. 47.)

Germ-Theory of Disease.—If the germ-theory of disease is believed, it is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease-germs which no bad air or foul drains can create, yet which may be pushed by foul air into virulent energy of reproduction. (Prof. Tyndall, p. 20.)

Intermittent Fever.—*Sulphate of Quinine.*—In treating simple intermittent fever by quinine we must take into account the intensity of the cause, also the duration of the complaint; for the more intense the first dose of the poison, and the older the case, the stronger must be the dose of quinine. We must also consider the type of the fever. Thus quartan fever requires a much larger dose of quinine than the quotidian or tertian type. Again, if we find the spleen enlarging we must at once increase the dose of the remedy. By attending to these rules our treatment of intermittents will be much more successful. In a general way the suitable dose of quinine varies between five and twenty-three grains. The minimum dose should be administered in recent cases of simple intermittent fever unattended by tumefaction of the spleen; the maximum in old cases of intermittent accompanied by considerable enlargement of spleen. The maximum dose must be commenced with immediately we see the case and continued for four or five days, when it may be decreased. Quinine is best tolerated by patients who are properly fed. (M. A. Monat, p. 18.)

Osteo-Arthritis, or Rheumatic Gout.—This disease results from a form of mal-assimilation in some way connected with a depressed state of vitality. The causes which produce it are all of a depressing nature. There is a remarkable antagonism between this disease and albuminuria—though the two never occur together, whilst in chronic gout the presence of albumen is not uncommon, and when chalk stones are present is almost constant. This is not unfrequently a valuable aid in the diagnosis, and even supposing albumen is not present in chronic gout the specific gravity of the urine is always below the average, whilst in rheumatic gout it is normal. The disease, on the other hand, is more liable to occur in members of consumptive families although the individual himself is almost exempt from liability to consumptive

diseases. Rheumatic gout must not be treated with colchicum and alkalies, which would depress and aggravate the disorder. Even in the acute form when the patient declares it is agony to move, and is bedewed with perspiration, he must be ordered to leave the bed, and if that does not check the perspiration the cold douche should be used for that purpose. Mineral acids with vegetable tonics, cod-liver oil, and a generous diet must be prescribed, and if the urine remains clear after standing, iron should be given, and a fair quantity of wine, beer, or porter. (Dr. H. W. Fuller, p. 52.)

Rheumatism and Gout.—It occasionally happens that during the exhibition of alkalies in cases of rheumatism or gout, and whilst there is a certain amount of alkalescence of urine, we have a copious deposit of phosphates. This is owing to depression, and is sure to be accompanied or followed by other symptoms of a corresponding kind. It is not necessary as might be supposed to give up or diminish the amount of alkali, for a few grains of quinine given daily will at once diminish the deposit, and enable the patient to persevere with the alkali. (Dr. A. W. Barclay, p. 126.)

Typhoid Fever.—*Mode of Propagation of.*—There is nothing to prove that typhoid fever is infectious by breathing air contaminated by the presence of a typhoid patient. Nor has it yet been proved that the poison is contained in, or disseminated, by, the exhalations from drains, privies, &c., however the breathing of air contaminated by such exhalations may predispose to the disease. *Almost* invariably the disease proceeds from a special poison contained in the alvine excreta, which is *directly* introduced into the alimentary canal either in the food, or, most generally, in the water drunk. Thus the physician, after examining the rose spots may in the course of a few minutes unconsciously pass his hand across his lips. The nurses do not observe always that scrupulous cleanliness so necessary in such cases, and thus the fever may be disseminated through a family by partaking of a meal. If there is any typhoid in a district, no surface-well water or pump water should be used at all. (Dr. P. W. Latham, p. 14.)

Relapses in Typhoid Fever.—Relapses almost always occur from giving solid food too early, that is before the ulcers in the ilium are healed. We have, however, in the thermometer an infallible guide. If the temperature of the patient at 8 a.m. and 6 p.m. has remained for two days at least about the normal point, or between 98° and 99° Fahr., solid food may be given, but not without. The tongue may be clean and moist, the appetite ravenous, yet if the temperature is not permanently at 98° the ulcers are still unhealed. (Dr. P. W. Latham, p. 17.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Hyperæsthesia.—Patients of the quasi-hysterical class, both male and female, are liable to suffer from hyperæsthesia of portions of the surface of the body, or even of the whole. The sensitive portion of skin will not bear the least touch; such a pressure as that of a stethoscope in auscultation would be intolerable. The reality of such suffering is materially confirmed by the fact that, in some cases of structural disease of the cord, certain parts of the cutaneous surface manifest intense reflex excitability, though ordinary sensation is much impaired. The throat, stomach, and even the tracheal and bronchial mucous surfaces are liable to hyperæsthesia. The heart enjoys no exemption. Cases are not at all rare where there is more or less præcordial uneasiness or pain, with rapid action of the heart: such cases are generally seen in young persons. What is known as "irritable uterus" is essentially a neurosis. The female breast and the joints are particularly liable to this affection, which is undoubtedly a condition of imperfect nutrition, not an exaltation but a deterioration of vital power. (Dr. C. H. Jones, p. 55.)

Neuralgic Headache.—*Turpentine.*—Turpentine in doses of one or two drachms, given in a little cold water, is an excellent remedy in cases of severe neuralgic headache, occurring in young persons of a delicate excitable temperament, without any menstrual or leucorrhœal complication. It is also of great use in the frontal headache, liable to occur after prolonged mental effort in either sex. A cup of very strong tea often relieves this form of headache, but it may produce general restlessness, and, worst of all, banish sleep. (Dr. J. W. Begbie, p. 79.)

Toothache.—*Chloral.*—A few grains of the solid hydrate introduced into the hollow cavity of a carious tooth will in most cases relieve toothache rapidly. A second or third application may be resorted to if necessary. (Dr. D. Page, p. 400.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Acupressure.—In ordinary circumstances, when acupressure has been skilfully performed, vessels of comparatively small size, such as the facial, temporal, radial, and ulnar, may be safely freed from acupressure in eight hours; and the larger arteries, as the humeral, axillary, and femoral, in twenty-four. The late Dr. Keith and Mr. Pirrie have altogether compressed upwards of two thousand arteries, and in only two cases has hemorrhage occurred on removal of the pin, and in each case the artery was again acupressed in a few seconds. It is

proved by dissection that the occlusion is effected by adhesion of the internal surfaces of the artery, and an internal coagulum more or less adherent to it. Union by the first intention is the aim of every surgeon, where accurate coaptation of surfaces and edges can be maintained. The superiority of acupressure over all other means of arresting hemorrhage yet devised consists in the removal of all local hindrance to early union within twenty-four hours of the operation. (Prof. Pirrie, p. 197.)

The Three Methods of Acupressure.—It was proposed by Mr. Pirrie, and agreed to by Sir James Simpson a short time before his death, that as only three methods of acupressure are necessary, these three methods only should be recommended by the friends of the operation, with the object of accelerating its more general adoption. These methods were named by Sir James Simpson, Circumclusion, Torsoclusion, and Retroclusion. *Circumclusion* is the well-known plan of a loop of fine wire passed over the point of the needle, and then, after compressing the artery, secured by twisting the ends round the needle. In *Torsoclusion*, a piece of tissue is taken up by inserting the pin close to the mouth of the bleeding artery, then pressing it onwards in a direction parallel to that of the vessel, and then causing its point to emerge. The second step consists in giving a quarter rotation to the pin, so as to place its emerged extremity above and at right angles to the artery; in pressing it well down against the small portion of tissue between the instrument and the vessel; and in sending the point for some distance into the tissues beyond the artery, for the purpose of securing the pin in position and of maintaining the twist. The artery is securely compressed by this plan, which may be described as the simplest, the easiest, and the quickest. *Retroclusion* is so named because the pin passes ultimately behind the artery. The pin being held almost flat upon the surface of the wound is made to take up a piece of tissue of small size, and the point then to emerge before reaching the artery. The head of the pin is then made to describe the greater part of a semicircle, so as to be placed nearly flat on the opposite side of the wound, its point is then sent behind the artery in the contrary direction to that in which it passed in the first movement, and pressed on until it is fixed in the tissues behind the vessel. (Prof. W. Pirrie, p. 194.)

A New Method of performing Acupressure.—The acupressure needle must be modified thus: About an inch from the point it must be twisted on itself, so as to form a small loop, and there must be another loop at the blunt end. The needle, as

a whole, is straight, except for the two loops. A piece of fine wire is then doubled in the middle and passed, first through the ring at the end of the pin, then through the loop on the staff. After sufficient has passed to reach easily over the point of the needle, the loop of wire should be bent up at right angles, so as to be out of the way. The needle is then ready for use. In using it, the point is first passed under the vessel, and then the loop of wire is bent down over the vessel and slipped over the point of the needle. By pulling on the free ends of the wire, and then twisting them round the needle close to the ring, the vessel is secured. To remove the wire ligature first untwist it, then pull a little on the pin point, and the loop of wire will glide along towards the point, and release the vessel. Should bleeding now take place, the vessel may again be compressed by pulling on the wire. After removal of the pin the wire follows. (Mr. R. C. Lucas, p. 201.)

ARTERY CONSTRICTOR.—*A new mode of Arresting Hemorrhage.*—In a case of amputation of the arm for injury, at Guy's Hospital, Mr. Bryant, instead of securing the artery by ligature or by torsion, which is the method chiefly employed at this hospital, used an instrument which had been brought to his notice by Dr. Fleet Speirs, of New York. This instrument is called the artery constrictor, and consists of a flattened metal tube, six inches, more or less, in length, open at both ends, with a sliding steel tongue running its whole length, and having a vice arrangement at one end, and a hook-shaped depression at the other, by which the artery can be grasped. It is then made to contract upon the artery by means of the vice at the upper end, which forces it within the sheath. In principle its action is the same as that of torsion, but the integrity of the external coat is more thoroughly preserved, while the continuity of the vessel is maintained. (Mr. T. Bryant, p. 178.)

DIGITALIS.—*Its Action on the Heart.*—Digitalis used to be considered a cardiac sedative, because it was found to relieve palpitation, which was thought to be over-action of the heart. It is now known, however, that there is no such thing as over-action, that hypertrophy is rather evidence of deficiency than of excess of power. It consequently follows, that if digitalis relieves palpitation it must be by a tonic action on the heart. It is in cases of deficient expulsive power of the heart walls that digitalis is chiefly of use; where the heart is distended, and in contraction only gets rid of a little blood off the top, remaining more or less full in systole. Under digitalis the contraction of the ventricles

becomes complete, and the pulse consequently steadier, firmer, and less compressible, the system is relieved, dyspnœa is abated, the deficient secretion of urine is improved, and free secretion takes its place. Dropsy is thus often relieved. (Dr. J. M. Fothergill, British Medical Journal, July 8, p. 27.)

The Use of Digitalis.—For persistent use digitalis should be given in powder. A favourite pill is the following: Half a grain to a grain of powdered digitalis, with an equal quantity of the dried sulphate of iron in powder, and a morsel of cayenne in extract of gentian or aloes and myrrh pill. Thus we secure at one cast an action on the circulation, the addition of iron, in a form which will act locally on the stomach, and thus act as an astringent on the gastric catarrh so common among the sufferers from heart disease. (Dr. J. M. Fothergill, British Medical Journal, Aug. 5, p. 148.)

Distrain of the Heart.—By "distrain of the heart," is meant a sudden dilatation of the heart without previous disease. That the heart may dilate slowly from long continued and excessive exertion is popularly recognised, but that it may yield suddenly, and in all directions, has not until recently been thought possible. Such, however, is the case, and with rapidly fatal results in most cases. The history is generally that, after severe exertion, intense pain occurs in the region of the heart, or in some part of the abdomen (very liable to mislead in diagnosis). The skin is at the same time cold and clammy, the face blue and congested, and the patient in a state of collapse. The pulse is small and rapid, and the heart's sounds extremely indistinct. After death the heart is found dilated, both cavities full of black blood, and both auriculo-ventricular apertures much enlarged. Should the patient recover, perfect rest is the most important remedial agent, and those medicines which quiet the action of the heart, such as conium, henbane, and belladonna, rather than digitalis, which is of little service. (Dr. R. Thompson, p. 100.)

Heart Disease.—*Heart disease from Strain in young people.*—A great number of cases of heart disease occur in large towns from strain and overwork amongst young people. These cases are quite unconnected with rheumatism, which has hitherto been considered almost exclusively the cause in early life. The actual pathological change produced is dilatation of one or both chambers of the heart, accompanied or not with hypertrophy, according to the general health and nourishment of the patient. If the heart overtaxed be a full one,

we find dilatation of both chambers. Prolonged rest will do a great deal for these cases, if the disease has not gone too far—but such hearts recover very slowly. After a time tricuspid regurgitation becomes established: this is as truly a disease as mitral regurgitation; for Dr. King's safety-valve hypothesis must not be believed in. When the right auriculo-ventricular orifice is once weakened, it is generally permanently so, for whenever the venous reflux in the external jugulars has been distinctly visible, it is always easily recalled in the same persons after long periods of rest. (Dr. T. C. Allbutt, p. 91.)

Disease of the Aorta from Over-exertion and Strain.—When a man is habitually engaged in a very laborious exertion, involving great strain on the heart and great vessels, the heart is able to accommodate itself to it by muscular growth, but with the aorta the case is very different; its resistance is great but its activity is nothing, or rather it is nothing more than the recoil of elastic fibre. Strain of such coats as these, so far from bringing gain of strength, brings loss of elasticity and weakness. Slight dilatation, with the occurrence of points of endo-arteritis, is the first change which takes place. From this point, pouching of the aorta, with consequent or concomitant incompetence of the valves, is quickly established. Such cases form the staple of heart diseases in the younger male patients of a hospital of a manufacturing town, and are totally unconnected with atheroma as a cause. (Dr. T. C. Allbutt, p. 99.)

State of Secretions in Heart Diseases.—A case of heart disease is related in which salines, with digitalis, squills, and other diuretics, had failed to give relief to the increasing anasarca, with scanty urine. On admission to hospital, it was found that the stools were extremely pale in colour, indicating a deficient action of the liver, and of the secreting apparatus of the bowels. Five grains of the compound digitalis pill was prescribed, containing three grains of blue pill, a grain of squills, and half a grain of digitalis, three times a day; and within three days the patient had begun to improve. By the end of a week the motions were a healthy colour, and then, the medicines which were useless before produced their proper effect—viz., a free secretion of urine, with great relief to the dyspnœa and anasarca. The case shows the great importance to successful treatment of examination of the secretions. (Dr. H. W. Fuller, p. 45.)

HEMOPTYSIS.—*Subcutaneous Injection of Ergotine.*—The author relates a case of hemoptysis which he treated by subcutane-

ous injection of five grains of ergotine dissolved in ten minims of distilled water. The hemorrhage returned twice subsequently. On all three occasions it was at once arrested by the ergotine. (Dr. W. A. Jamieson, p. 108.)

HEMORRHOIDS.—A gentleman of good health suffered severely from hemorrhoids. Being told that if he starved himself he would obtain a cure without any operation, he did so. For weeks he kept himself at starvation point, or nearly so, and the tumours all became absorbed—nor has he been troubled with them since. (Mr. C. de Morgan, p. 39.)

LIGATURE.—*New Method of Ligaturing Bleeding Vessels.*—There is no doubt that ligature is by far the safest method of arresting hemorrhage from a divided artery, inasmuch as a ligatured vessel will resist the pressure of a column of mercury 114 inches high, whilst those closed by acupressure or torsion only resist a column 23 and 13 inches high respectively. Still acupressure and torsion have the great advantage of not necessitating the retention in the wound of a body directly provocative of suppuration. The safety of ligature may, however, be combined with the advantages of the other two methods by the use of a peculiar kind of knot. This knot, whilst it is perfectly secure, only requires traction on the end of the ligature to allow of its complete and easy withdrawal from the wound. The knot will be at once understood by reference to the woodcut at p. 205. (Dr. A. Ogston, p. 203.)

LIGATURE OF THE SUBCLAVIAN ARTERY.—*A New Incision.*—In the ordinary operation for ligature of the subclavian artery the incision is made close to and parallel with the clavicle, and the guides to the artery are the edge of the scalenus anticus and the tubercle on the first rib. But unfortunately it is often impossible to feel the edge of the muscle in a bleeding wound, and the tubercle on the first rib is not always so well developed as to permit of recognition by the sense of touch. The only real guide to the artery is the lower edge of the omo-hyoid muscle. Make the incision, beginning at the posterior edge of the sterno-mastoid muscle, one inch and a quarter above the clavicle, and ending within a quarter of an inch of the attachment of the trapezius to that bone, dividing skin and platysma. This incision should be a little short of three inches. The external jugular should then be ligatured in two places and divided, the deep cervical fascia then divided, and the posterior belly of the omo-hyoid will appear on retraction of the edges of the wound upwards and downwards. The white cords of the plexus, with the artery inferior and internal to them, will be seen to occupy the

bottom of the wound. The knife is then to be laid aside, and the vessel ligatured in the usual way. (Mr F. P. Staples, p. 208.)

Popliteal Aneurism.—*Compression.*—The compressing power, whatever it is, provided that it just arrests the circulation in the vessel, and is elastic, should always be commenced at 9 a.m., and left off during the night, so as to allow of uninterrupted sleep, whereby the health of the patient is kept up to the condition most favourable for fibrinous deposit in the sac. Any force employed exceeding that which would be represented by a *dead* pressure of nine pounds is too great, as the pain to which it will give rise will soon render it intolerable. (Dr. R. Macnamara, p. 210.)

Valvular Murmurs.—In giving an opinion as to the prognosis of a case of heart-disease accompanied with valvular murmur great care is necessary. We are very liable to be misled if we take the murmur as our sole guide, both as to the presence of disease and as to its real importance. In the first place functional murmurs may occur at the apex as well as the base of the heart, and be so intense as in the closest manner to make the case resemble serious organic disease. Apart from the general history of the case the position in which the apex of the heart strikes the chest-wall is our best guide, for if there is valvular lesion hypertrophy results, and the point of impulse is considerably lowered. It is wonderful also to what an extent the lesions upon which some murmurs depend admit of repair. Reparative action takes place, very slowly however, provided that the heart has rest. (Dr. H. W. Fuller, p. 104.)

Varicose Veins.—*Application of strong Nitric Acid.*—Varicose tumours may be successfully treated as follows:—Suppose, for example, that the tumour is on the leg. Pressure having been made above and below the tumour, the integuments must be raised from it, and an incision by transfixion made in the skin so as to expose the tumour. The fuming nitric acid is then to be applied to the external coat of the vein. No pain will attend this application. On the following day the contents of the tumour will appear solidified, and the acid may be applied again. The process of solidification will then go on rapidly, the tumour at the same time decreasing in size. In the course of a week a small slough will come away, leaving a wound which may be dressed with tinct. benzoin co. and glycerine. This practice was suggested by Sir Dominic Corrigan, to whom the idea occurred from the success which generally attends the treatment of hemorrhoidal tumours by nitric acid. (Mr. Stokes, p. 214.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASTHMA.—*Nitrite of Amyl.*—Nitrite of amyl is a remedy of the greatest value. Its importance has not yet been recognised. It should be given by inhalation in doses of five minims, on a piece of lint laid over the nostrils. It is a paralyser of the organic nerves which control the size of the blood-vessels, which, consequently, under its action dilate from the pressure of blood within. It is also highly probable that it acts upon the unstriped muscular fibre of the body, generally producing relaxation. The first symptom of its action is acceleration of the action of the heart, or flushing of the face. There is also warmth of head, face, and neck, and perspiration: the warmth and perspiration often being general. The pulse is the first tell-tale of its effects: in from eight to twelve seconds its frequency rapidly increases, even to double its ordinary rate. The beating of the heart and carotids is in some persons very marked. Its action in a case of spasmodic asthma is magical, and it may also be used with advantage in cardiac dyspnœa, angina pectoris. It seems likely to be of great service in attacks of laryngismus stridulus. It may be inhaled directly from the bottle neck. (Dr. T. Jones, Practitioner, Aug. 1, p. 213.)

CHRONIC BRONCHITIS.—If a little cotton wool is placed in front of the mouth, and slightly moistened to make it adhere, it will be found to prolong sleep, abate the irritation of the throat, and greatly mitigate a hacking cough. (Prof. Tyndall, p. 25.)

PARACENTESIS THORACIS.—As soon as ever it is ascertained that the absorption of pleuritic fluid is improbable, it is better to draw it off at once, otherwise the lung becomes bound down permanently by adhesions, or the fluid becomes purulent, and the patient worn down by fever and hectic. The admission of a little air during the operation, whatever it may be in theory, is found in practice to be of no consequence whatever. The suction trochars and other contrivances of a like kind are quite unnecessary. The chest should be tapped as low down as possible, and a free opening should be made, so as to allow of the chest emptying itself thoroughly. If the fluid is found to be purulent, some means should be adopted to prevent the wound from closing, in order that the matter may drain off as soon as it is formed. The patient should be supported after the operation by bark and good nourishment, and for a day or two opium should be given if necessary. (Dr. H. W. Fuller, p. 214.)

ULCERATED THROAT AND CAVITY IN THE LUNG.—*Inhalation of Iodine.*—The readiest and most effectual means of apply-

ing this practice is to use a solution of twenty grains of iodine to the ounce of hydride of amyl (a solution of great service in other cases), and to dilute a portion of this with more of the hydride until the vapour of the iodine given is scarcely at all irritating to the throat. This solution may be inhaled from a little funnel of parchment paper, holding in it some finely-teazed cotton-wool, on which the solution may be dropped. It should be held a little way from the nostrils and mouth, so as to allow the admission of plenty of fresh air. The quantity of solution used should be so regulated that about five grains of iodine are inhaled at a time. (Dr. B. W. Richardson, p. 378.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Cholera.—If cholera invades this country the main objects for endeavour must be to secure such local circumstances that cholera-contagium, though not disinfected, shall be unable to act extensively on the population. There are two dangers which have to be guarded against as favouring the spread of cholera-contagium. First, and above all, there is the danger of water supplies which are in any (even the slightest) degree tainted by house refuse or other light kinds of filth; and secondly, there is the danger of breathing air which is foul with effluvia from the same sorts of impurity. Happily for mankind, cholera is so little contagious that, if reasonable care be taken, there is but little risk that the disease will spread to those who nurse and attend the sick. All the matters, however, which the patient discharges from the stomach and bowels are infective. They should be immediately disinfected, or their power of spreading the disease increases more and more for some days. The infective power of choleraic discharges likewise attaches to whatever bedding, clothes, or linen have been imbued with them. (Mr. Simon, Lancet, Aug. 19, p. 279.)

Calabar Bean.—Calabar bean has the peculiar effect of causing dilatation of the minute arteries and the capillaries. This appears to be the very thing wanted in cholera, in which these vessels are empty and contracted, the blood being driven into the veins. It not only opposes the cholera poison in this particular, but it raises the temperature of the external parts of the body, which are depressed in that disease. It should be given by subcutaneous injection, in doses of $\frac{1}{8}$th grain of the extract every two hours. This plan of treatment is simply a suggestion, as it has not been put to the test of practice, but as it would not interfere with other treatment, there seems no reason why it should not be. (Dr. Munro, Edinburgh Med. Journal, Oct., p. 327.)

Collapse of Cholera.—*Nitrite of Amyl.*—It is now pretty clearly proved that the collapse of cholera is owing to an impediment to the circulation of blood through the lungs by the contraction of the muscular fibres of the minute pulmonary arteries. This is just the condition which nitrite of amyl is most calculated to relieve. It gives instant relief in asthmatic collapse, and its administration invariably causes increased frequency of cardiac pulsation, dilatation of the arterioles, flushing of the face, warmth of body, and perspiration. It should be administered by inhalation. Its effects have yet to be put to the test of experience in cholera, but, so far as we can judge from our present knowledge of its action in other diseases, it is likely to prove of great value. (Dr. Talfourd Jones, Brit. Med. Journal, Sept. 30, p. 378).

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Lithotomy or Lithotrity?—The most important element in deciding which operation is preferable in any given case, is the size of the stone. A stone which measures $1\frac{1}{4}$ inches to $1\frac{3}{8}$ inches, in its longest diameter, is for the most part fairly amenable to lithotrity. A stone of which any measurement is $1\frac{3}{4}$ inches is mostly too large. The exact measurement is easily determinable, by catching the stone first in one diameter and then in the other, between the blades of a flat-bladed lithotrite, the size may be read off at the handle. The next thing is the nature of the stone, for an oxalate of lime calculus cannot be crushed, and must be removed by lithotomy. The feel of an oxalate of lime calculus is wholly different from that of a uric acid or phosphatic one, and when seized by the blades of the lithotrite experimentally, it does not "give" in the least, but the blades recoil from it with a spring, whereas, in the other two forms of calculus, a slight impression is made, and the blades feel to bite, or prick a little in the surface of the stone. The third

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Ankylosis of Joints.—If it is decided that a fibrous ankylosis is too firm to yield to gradual extension, even with the aid of tenotomy, it must be treated by sudden rupture. This operation is not attended with risk of inflammation or any bad consequences, provided the following precautions are observed. All contracted tendons must be divided and the punctures healed before the operation, or they may extend into rents of the skin of large size. Chloroform having been administered, the limb must be firmly fixed, so that all motion is prevented, except that which the operator is about to impart to the limb. The adhesions must then be ruptured instantaneously by force applied in the direction of flexion. The limb, and especially the joint, is then to be firmly bandaged and confined by a gutta-percha or flexible splint. The bandage should on no account be removed until all tenderness has ceased. It seems impossible, with these precautions, to set up unhealthy action. (Mr. B. E. Brodhurst, p. 139.)

Antiseptic Surgery.—The discovery of Prof. Tyndall that such substances as cotton-wool possess the power of filtering the air passed through them from the invisible dust always

suspended in the atmosphere, has given a new impulse to antiseptic surgery. It naturally follows that if a wound is covered with a good thickness of cotton-wool the further access of putrefactive particles or germs may be prevented. The cotton itself, however, requires thoroughly freeing from such particles, by being impregnated with the vapour of carbolic acid, to the extent of about the two hundredth part of its weight. The surface of the sore to be dressed should first be washed with a solution of carbolic acid, one part in forty, and then covered with a piece of oiled silk, the size of the sore, to prevent the dressings sticking through dryness. Over this must be placed a piece of folded linen rag, rather larger than the oiled silk, and impregnated with the carbolic acid vapour, in the same manner as the cotton-wool; the object of the rag being to absorb the discharge and prevent it from trickling down below the slightly-absorbent cotton, and so causing spread of putrefaction to the wound. Lastly, a well-overlapping mass of the carbolised cotton-wool is to be securely fixed by a bandage. By the cotton-wool all putrefaction will be excluded from the wound, although all chemical antiseptic power has left it. (Prof. Lister, p. 139.)

Antiseptic Cotton Gauze.—The material now used as an antiseptic dressing by Prof. Lister is a loose cotton fabric, the fibres of which are impregnated with carbolic acid securely lodged in insoluble resin, which holds the carbolic acid with remarkable tenacity, while at the same time a little paraffin is added to prevent the adhesiveness which the mixture of carbolic acid and resin would otherwise possess. The interstices between the fibres are kept free from these ingredients, so that the fabric may be fitted for absorbing discharges. This is laid on the wound in a thickness of about eight layers, and having a piece of impermeable tissue, such as what is known as "hat lining," interposed between the two outer layers. This compels the discharge to traverse all the extent of the tissue instead of passing directly outwards through it. Of course all septic particles must be destroyed in the wound itself before this dressing is applied. (Prof. Lister, p. 132.)

Antiseptic Spray during Operations.—In operating upon a case where the integument is previously unbroken, the entrance of all septic particles should be prevented by playing upon the part with a spray of carbolic acid and water. This need only be of the strength of one part of the acid to 100 parts of water, and not one part to 40 as previously recommended. The spray should be made as fine as possible, by having the lower end of the water tube almost entirely stopped up, leaving the air tube as before. (Prof. Lister, p. 133.)

Bandaging.—*An Aid to.*—A solution of wax in hydride of amyl is an excellent thing to pour over a bandage after being applied, or in which to saturate it after application. The hydride evaporates with great rapidity, and leaves the firm but flexible casing of bandage and wax. (Dr. B. W. Richardson, p. 382.)

Drop-Wrist from Injury to thf Musculo-Spiral Nerve in Cases of Fracture of the Humerus.—Occasionally in fracture of the shaft of the humerus the musculo-spiral nerve which winds closely round it is injured, and consequently we have paralysis of the extensors of the wrist and fingers, and of the supinators of the forearm. The appearance is a drop-wrist exactly like that resulting from lead-poisoning. Lower down it divides and comes into close relation with the outer condyle, injury of which also is liable to be accompanied by injury to the nerve or one of its divisions. The two divisions of the nerve are the radial and posterior interosseous. Sometimes the latter is the only nerve injured, and then the paralysis is not so complete, for the extensor carpi radialis longior and supinator longus are supplied by branches from the main trunk, and are consequently not paralysed. The best remedy in our hands is the persevering employment of faradisation to the arm, along with a splint specially constructed to produce extension of the fingers. (Prof. Erichsen, p. 188.)

Excision of Joints.—*When should a Joint be Excised?*—The state of the general health should primarily determine the necessity for excision in all cases, and not any arbitrary consideration of the period of the disease and the condition of the joint. Whenever therefore the general health is manifestly failing, whatever may be the stage of the disease, excision should be resorted to, without further delay. Osseous anchylosis with malposition of the limb will *not* justify the peril of attempted excision. (Mr. F. J. Gant, p. 163.)

Excision of the Elbow-Joint.—It not unfrequently happens that a spiculated enlargement, or growth of new bone occurs, just about the diseased portion of bone, and limiting it; this must not be removed in excision of diseased joint. The bone should be removed up to this point, but not beyond it. (Mr. F. J. Gant, p. 175.)

Excision of the Wrist.—To perform this operation, make a curvilinear incision extending from just above the styloid process of the radius downwards across the back of the wrist and upwards to the same level above the styloid process of the ulna. The flap of integument is now to be reflected, carefully avoiding the extensor tendons of the fingers, and those of the thumb, on the ulnar half of the radius. Then,

dividing the supinator tendons, and the extensor tendons of the carpus, and flexing the wrist, the radio-carpal articulation is to be opened. The other extensor tendons referred to must then be drawn aside with a curved spatula, by an assistant, and the articular ends of the radius and ulna, the carpel bones, and bases of the metacarpus, are successively removed by a small saw or cutting pliers introduced transversely. There is another method by means of two lateral longitudinal incisions, but there are several objectionable points to it. In the after treatment it is very important to maintain flexibility of the fingers and thumb by working them daily. (Mr. F. J. Gant, p. 179.)

Excision of the Ankle-Joint for Disease.—This operation may be said to have been introduced into British Surgery by Mr. Hancock, in 1851, although performed for injury in 1766 by Mr. Hey, of Leeds. It is an operation of value provided suitable cases only are chosen. It should be performed, 1, when disease commencing in the synovial membrane has extended to and destroyed the articular surfaces of the tibia and fibula, that of the astragalus, or of both opposed surfaces; 2, when disease, having the same articular consequences, commenced in the cancellated tissue either of the long bones or of astragalus, provided it be limited to part of this bone—its upper articular portion. If resorted to at all excision should always be performed before the supervention of constitutional exhaustion. (Mr. F. J. Gant, p. 179.)

External Tumours.—*Michel's Process for Removing.*—The following is Michel's process for removing external tumours, for which Dr. Bell gave no less a sum than 25,000 francs. Asbestos in an impalpable powder is mixed with three times its weight of strong sulphuric acid. A mass is thus formed which may be easily worked with a silver or gold spatula into any size or shape corresponding to the tumour to be destroyed. Before its application the adjoining healthy parts of the skin must be carefully protected by a zone of collodion and pads of linen, and the patient must be so placed that the surface of the tumour is perfectly level. The tumour is usually destroyed by two applications of the caustic, the first of which must be of the thickness of half an inch for a tumour the size of a hen's egg. The second application should be smaller, and about twelve hours after the first. Hardly any pain is caused after the first half-hour. (Dr. W. A. Bell, p. 186.)

Fracture of the Patella.—The cause of separation of the two portions of a fractured patella is not, as is generally supposed, contraction of the rectus muscle, but effusion within

the joint. There is, perhaps, no more successful way of treating these cases than to apply strips of plaster from below the lower fragment upwards, so as to fix this portion of the bone, and other strips from above the upper fragment downwards, so as to bring this portion of the bone downwards to the lower fragment. The limb should be allowed to be flat upon the bed. (Mr. J. Hutchinson, p. 167.)

HATTERS'-FELT SPLINTS.—Stout hatters'-felt, such as is used for making the hats of the Cornish miners, has for some time been largely employed as a material for splints at the London Hospital. It is particularly suitable for use after excision of the knee-joint. It is readily cut and moulded to the limb after soaking in hot water, and is capable of easy adjustment with iron brackets, &c., where such appliances are required. They are firm and yet light. (Mr. J. Hutchinson, p. 166.)

HIP-JOINT DISEASE.—*Dr. Sayre's Method of Treating.*—The author regards the majority of cases of hip-joint disease as the result, not of a strumous or tubercular dyscrasia, but of some local cause—a blow, or twist, or sprain. The resulting slight inflammation of synovial membrane, not being subdued by immediate rest, goes on developing slowly into a grave disease. He considers that purely local means of treatment are required, and that the treatment by long-continued rest and antiscrofulous medicinal agents is not only unnecessary but injurious. Dr. S. employs a splint by means of which slight extension of the joint is kept up, and the adaptation of which, according to the evidence of the editor of the *Medical Times and Gazette*, who saw it applied at the Middlesex Hospital, at once relieved all the pain in the case of a little boy in the second stage of the disease. For the description of this splint and mode of its application refer to the original paper. (Dr. L. A. Sayre, p. 148.)

Disease of the Hip-Joint in Children.—The great error we commit is in looking upon this disease as necessarily one of constitutional blood-disease, of which the joint affection is merely a local manifestation. We ought in the treatment to let the local affection be our sole source of care, for the disease is in no way connected with scrofula, nor is there any tuberculous disease of the bone. The reason these cases go on so unfavourably is that, owing to muscular contraction, the head of the femur is kept constantly pressed against the acetabulum, and absorption of both surfaces of bone takes place in consequence; there is destruction of bone, terminating in an abscess, which finds an outlet somewhere; or else it goes on to ankylosis. Now it will be found that

bottom of the wound. The knife is then to be laid aside, and the vessel ligatured in the usual way. (Mr F. P. Staples, p. 208.)

Popliteal Aneurism.—*Compression.*—The compressing power, whatever it is, provided that it just arrests the circulation in the vessel, and is elastic, should always be commenced at 9 a.m., and left off during the night, so as to allow of uninterrupted sleep, whereby the health of the patient is kept up to the condition most favourable for fibrinous deposit in the sac. Any force employed exceeding that which would be represented by a *dead* pressure of nine pounds is too great, as the pain to which it will give rise will soon render it intolerable. (Dr. R. Macnamara, p. 210.)

Valvular Murmurs.—In giving an opinion as to the prognosis of a case of heart-disease accompanied with valvular murmur great care is necessary. We are very liable to be misled if we take the murmur as our sole guide, both as to the presence of disease and as to its real importance. In the first place functional murmurs may occur at the apex as well as the base of the heart, and be so intense as in the closest manner to make the case resemble serious organic disease. Apart from the general history of the case the position in which the apex of the heart strikes the chest-wall is our best guide, for if there is valvular lesion hypertrophy results, and the point of impulse is considerably lowered. It is wonderful also to what an extent the lesions upon which some murmurs depend admit of repair. Reparative action takes place. very slowly however, provided that the heart has rest. (Dr. H. W. Fuller, p. 104.)

Varicose Veins.—*Application of strong Nitric Acid.*—Varicose tumours may be successfully treated as follows:—Suppose, for example, that the tumour is on the leg. Pressure having been made above and below the tumour, the integuments must be raised from it, and an incision by transfixion made in the skin so as to expose the tumour. The fuming nitric acid is then to be applied to the external coat of the vein. No pain will attend this application. On the following day the contents of the tumour will appear solidified, and the acid may be applied again. The process of solidification will then go on rapidly, the tumour at the same time decreasing in size. In the course of a week a small slough will come away, leaving a wound which may be dressed with tinct. benzoin co. and glycerine. This practice was suggested by Sir Dominic Corrigan, to whom the idea occurred from the success which generally attends the treatment of hemorrhoidal tumours by nitric acid. (Mr. Stokes, p. 214.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASTHMA.—*Nitrite of Amyl.*—Nitrite of amyl is a remedy of the greatest value. Its importance has not yet been recognised. It should be given by inhalation in doses of five minims, on a piece of lint laid over the nostrils. It is a paralyser of the organic nerves which control the size of the blood-vessels, which, consequently, under its action dilate from the pressure of blood within. It is also highly probable that it acts upon the unstriped muscular fibre of the body, generally producing relaxation. The first symptom of its action is acceleration of the action of the heart, or flushing of the face. There is also warmth of head, face, and neck, and perspiration: the warmth and perspiration often being general. The pulse is the first tell-tale of its effects: in from eight to twelve seconds its frequency rapidly increases, even to double its ordinary rate. The beating of the heart and carotids is in some persons very marked. Its action in a case of spasmodic asthma is magical, and it may also be used with advantage in cardiac dyspnœa, angina pectoris. It seems likely to be of great service in attacks of laryngismus stridulus. It may be inhaled directly from the bottle neck. (Dr. T. Jones, Practitioner, Aug. 1, p. 213.)

CHRONIC BRONCHITIS.—If a little cotton wool is placed in front of the mouth, and slightly moistened to make it adhere, it will be found to prolong sleep, abate the irritation of the throat, and greatly mitigate a hacking cough. (Prof. Tyndall, p. 25.)

PARACENTESIS THORACIS.—As soon as ever it is ascertained that the absorption of pleuritic fluid is improbable, it is better to draw it off at once, otherwise the lung becomes bound down permanently by adhesions, or the fluid becomes purulent, and the patient worn down by fever and hectic. The admission of a little air during the operation, whatever it may be in theory, is found in practice to be of no consequence whatever. The suction trochars and other contrivances of a like kind are quite unnecessary. The chest should be tapped as low down as possible, and a free opening should be made, so as to allow of the chest emptying itself thoroughly. If the fluid is found to be purulent, some means should be adopted to prevent the wound from closing, in order that the matter may drain off as soon as it is formed. The patient should be supported after the operation by bark and good nourishment, and for a day or two opium should be given if necessary. (Dr. H. W. Fuller, p. 214.)

ULCERATED THROAT AND CAVITY IN THE LUNG.—*Inhalation of Iodine.*—The readiest and most effectual means of apply-

ing this practice is to use a solution of twenty grains of iodine to the ounce of hydride of amyl (a solution of great service in other cases), and to dilute a portion of this with more of the hydride until the vapour of the iodine given is scarcely at all irritating to the throat. This solution may be inhaled from a little funnel of parchment paper, holding in it some finely-teazed cotton-wool, on which the solution may be dropped. It should be held a little way from the nostrils and mouth, so as to allow the admission of plenty of fresh air. The quantity of solution used should be so regulated that about five grains of iodine are inhaled at a time. (Dr. B. W. Richardson, p. 378.)

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Cholera.—If cholera invades this country the main objects for endeavour must be to secure such local circumstances that cholera-contagium, though not disinfected, shall be unable to act extensively on the population. There are two dangers which have to be guarded against as favouring the spread of cholera-contagium. First, and above all, there is the danger of water supplies which are in any (even the slightest) degree tainted by house refuse or other light kinds of filth; and secondly, there is the danger of breathing air which is foul with effluvia from the same sorts of impurity. Happily for mankind, cholera is so little contagious that, if reasonable care be taken, there is but little risk that the disease will spread to those who nurse and attend the sick. All the matters, however, which the patient discharges from the stomach and bowels are infective. They should be immediately disinfected, or their power of spreading the disease increases more and more for some days. The infective power of choleraic discharges likewise attaches to whatever bedding, clothes, or linen have been imbued with them. (Mr. Simon, Lancet, Aug. 19, p. 279.)

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Ankylosis of Joints.—If it is decided that a fibrous ankylosis is too firm to yield to gradual extension, even with the aid of tenotomy, it must be treated by sudden rupture. This operation is not attended with risk of inflammation or any bad consequences, provided the following precautions are observed. All contracted tendons must be divided and the punctures healed before the operation, or they may extend into rents of the skin of large size. Chloroform having been administered, the limb must be firmly fixed, so that all motion is prevented, except that which the operator is about to impart to the limb. The adhesions must then be ruptured instantaneously by force applied in the direction of flexion. The limb, and especially the joint, is then to be firmly bandaged and confined by a gutta-percha or flexible splint. The bandage should on no account be removed until all tenderness has ceased. It seems impossible, with these precautions, to set up unhealthy action. (Mr. B. E. Brodhurst, p. 139.)

Antiseptic Surgery.—The discovery of Prof. Tyndall that such substances as cotton-wool possess the power of filtering the air passed through them from the invisible dust always

suspended in the atmosphere, has given a new impulse to antiseptic surgery. It naturally follows that if a wound is covered with a good thickness of cotton-wool the further access of putrefactive particles or germs may be prevented. The cotton itself, however, requires thoroughly freeing from such particles, by being impregnated with the vapour of carbolic acid, to the extent of about the two hundredth part of its weight. The surface of the sore to be dressed should first be washed with a solution of carbolic acid, one part in forty, and then covered with a piece of oiled silk, the size of the sore, to prevent the dressings sticking through dryness. Over this must be placed a piece of folded linen rag, rather larger than the oiled silk, and impregnated with the carbolic acid vapour, in the same manner as the cotton-wool; the object of the rag being to absorb the discharge and prevent it from trickling down below the slightly-absorbent cotton, and so causing spread of putrefaction to the wound. Lastly, a well-overlapping mass of the carbolised cotton-wool is to be securely fixed by a bandage. By the cotton-wool all putrefaction will be excluded from the wound, although all chemical antiseptic power has left it. (Prof. Lister, p. 139.)

Antiseptic Cotton Gauze.—The material now used as an antiseptic dressing by Prof. Lister is a loose cotton fabric, the fibres of which are impregnated with carbolic acid securely lodged in insoluble resin, which holds the carbolic acid with remarkable tenacity, while at the same time a little paraffin is added to prevent the adhesiveness which the mixture of carbolic acid and resin would otherwise possess. The interstices between the fibres are kept free from these ingredients, so that the fabric may be fitted for absorbing discharges. This is laid on the wound in a thickness of about eight layers, and having a piece of impermeable tissue, such as what is known as "hat lining," interposed between the two outer layers. This compels the discharge to traverse all the extent of the tissue instead of passing directly outwards through it. Of course all septic particles must be destroyed in the wound itself before this dressing is applied. (Prof. Lister, p. 132.)

Antiseptic Spray during Operations.—In operating upon a case where the integument is previously unbroken, the entrance of all septic particles should be prevented by playing upon the part with a spray of carbolic acid and water. This need only be of the strength of one part of the acid to 100 parts of water, and not one part to 40 as previously recommended. The spray should be made as fine as possible, by having the lower end of the water tube almost entirely stopped up, leaving the air tube as before. (Prof. Lister, p. 133.)

Bandaging.—*An Aid to.*—A solution of wax in hydride of amyl is an excellent thing to pour over a bandage after being applied, or in which to saturate it after application. The hydride evaporates with great rapidity, and leaves the firm but flexible casing of bandage and wax. (Dr. B. W. Richardson, p. 382.)

Drop-Wrist from Injury to the Musculo-Spiral Nerve in Cases of Fracture of the Humerus.—Occasionally in fracture of the shaft of the humerus the musculo-spiral nerve which winds closely round it is injured, and consequently we have paralysis of the extensors of the wrist and fingers, and of the supinators of the forearm. The appearance is a drop-wrist exactly like that resulting from lead-poisoning. Lower down it divides and comes into close relation with the outer condyle, injury of which also is liable to be accompanied by injury to the nerve or one of its divisions. The two divisions of the nerve are the radial and posterior interosseous. Sometimes the latter is the only nerve injured, and then the paralysis is not so complete, for the extensor carpi radialis longior and supinator longus are supplied by branches from the main trunk, and are consequently not paralysed. The best remedy in our hands is the persevering employment of faradisation to the arm, along with a splint specially constructed to produce extension of the fingers. (Prof. Erichsen, p. 188.)

Excision of Joints.—*When should a Joint be Excised?*—The state of the general health should primarily determine the necessity for excision in all cases, and not any arbitrary consideration of the period of the disease and the condition of the joint. Whenever therefore the general health is manifestly failing, whatever may be the stage of the disease, excision should be resorted to, without further delay. Osseous anchylosis with malposition of the limb will *not* justify the peril of attempted excision. (Mr. F. J. Gant, p. 163.)

Excision of the Elbow-Joint.—It not unfrequently happens that a spiculated enlargement, or growth of new bone occurs, just about the diseased portion of bone, and limiting it; this must not be removed in excision of diseased joint. The bone should be removed up to this point, but not beyond it. (Mr. F. J. Gant, p. 175.)

Excision of the Wrist.—To perform this operation, make a curvilinear incision extending from just above the styloid process of the radius downwards across the back of the wrist and upwards to the same level above the styloid process of the ulna. The flap of integument is now to be reflected, carefully avoiding the extensor tendons of the fingers, and those of the thumb, on the ulnar half of the radius. Then,

dividing the supinator tendons, and the extensor tendons of the carpus, and flexing the wrist, the radio-carpal articulation is to be opened. The other extensor tendons referred to must then be drawn aside with a curved spatula, by an assistant, and the articular ends of the radius and ulna, the carpel bones, and bases of the metacarpus, are successively removed by a small saw or cutting pliers introduced transversely. There is another method by means of two lateral longitudinal incisions, but there are several objectionable points to it. In the after treatment it is very important to maintain flexibility of the fingers and thumb by working them daily. (Mr. F. J. Gant, p. 179.)

Excision of the Ankle-Joint for Disease.—This operation may be said to have been introduced into British Surgery by Mr. Hancock, in 1851, although performed for injury in 1766 by Mr. Hey, of Leeds. It is an operation of value provided suitable cases only are chosen. It should be performed, 1, when disease commencing in the synovial membrane has extended to and destroyed the articular surfaces of the tibia and fibula, that of the astragalus, or of both opposed surfaces; 2, when disease, having the same articular consequences, commenced in the cancellated tissue either of the long bones or of astragalus, provided it be limited to part of this bone—its upper articular portion. If resorted to at all excision should always be performed before the supervention of constitutional exhaustion. (Mr. F. J. Gant, p. 179.)

External Tumours.—*Michel's Process for Removing.*—The following is Michel's process for removing external tumours, for which Dr. Bell gave no less a sum than 25,000 francs. Asbestos in an impalpable powder is mixed with three times its weight of strong sulphuric acid. A mass is thus formed which may be easily worked with a silver or gold spatula into any size or shape corresponding to the tumour to be destroyed. Before its application the adjoining healthy parts of the skin must be carefully protected by a zone of collodion and pads of linen, and the patient must be so placed that the surface of the tumour is perfectly level. The tumour is usually destroyed by two applications of the caustic, the first of which must be of the thickness of half an inch for a tumour the size of a hen's egg. The second application should be smaller, and about twelve hours after the first. Hardly any pain is caused after the first half-hour. (Dr. W. A. Bell, p. 186.)

Fracture of the Patella.—The cause of separation of the two portions of a fractured patella is not, as is generally supposed, contraction of the rectus muscle, but effusion within

the joint. There is, perhaps, no more successful way of treating these cases than to apply strips of plaster from below the lower fragment upwards, so as to fix this portion of the bone, and other strips from above the upper fragment downwards, so as to bring this portion of the bone downwards to the lower fragment. The limb should be allowed to be flat upon the bed. (Mr. J. Hutchinson, p. 167.)

HATTERS'-FELT SPLINTS.—Stout hatters'-felt, such as is used for making the hats of the Cornish miners, has for some time been largely employed as a material for splints at the London Hospital. It is particularly suitable for use after excision of the knee-joint. It is readily cut and moulded to the limb after soaking in hot water, and is capable of easy adjustment with iron brackets, &c., where such appliances are required. They are firm and yet light. (Mr. J. Hutchinson, p. 166.)

HIP-JOINT DISEASE.—*Dr. Sayre's Method of Treating.*—The author regards the majority of cases of hip-joint disease as the result, not of a strumous or tubercular dyscrasia, but of some local cause—a blow, or twist, or sprain. The resulting slight inflammation of synovial membrane, not being subdued by immediate rest, goes on developing slowly into a grave disease. He considers that purely local means of treatment are required, and that the treatment by long-continued rest and antiscrofulous medicinal agents is not only unnecessary but injurious. Dr. S. employs a splint by means of which slight extension of the joint is kept up, and the adaptation of which, according to the evidence of the editor of the *Medical Times and Gazette*, who saw it applied at the Middlesex Hospital, at once relieved all the pain in the case of a little boy in the second stage of the disease. For the description of this splint and mode of its application refer to the original paper. (Dr. L. A. Sayre, p. 148.)

Disease of the Hip-Joint in Children.—The great error we commit is in looking upon this disease as necessarily one of constitutional blood-disease, of which the joint affection is merely a local manifestation. We ought in the treatment to let the local affection be our sole source of care, for the disease is in no way connected with scrofula, nor is there any tuberculous disease of the bone. The reason these cases go on so unfavourably is that, owing to muscular contraction, the head of the femur is kept constantly pressed against the acetabulum, and absorption of both surfaces of bone takes place in consequence; there is destruction of bone, terminating in an abscess, which finds an outlet somewhere; or else it goes on to ankylosis. Now it will be found that

although ordinary movement of the limb causes exquisite torture, that if it is slightly extended it may be moved about without causing any pain whatever. In treating a case, put the child to bed, and apply extension of the limb in the direction of the angle of deformity as you find it. (If the capsule is distended with fluid, as in the early stages of the disease, the leg will be abducted; if the capsule is ruptured, it will be adducted.) In the course of a week or ten days there will be so much improvement that the limb may be brought straight, extension being still continued. For the details of this treatment refer to the article at page 155. (Dr. L. A. Sayre, p. 147.)

WOUNDS.—*Modified Antiseptic Treatment.*—Dr. J. S. Walker, of Hanley, recommends a modification of the antiseptic treatment. His treatment after amputation of the arm or leg, may be taken as illustrative. His directions are:—Sponge out the wound thoroughly with chloride of zinc ʒ ss. to ℥ j. of water, then let all applications be quite dry for the first two or three days; as soon as there is the least secretion of pus, take a Higginson's syringe and wash out the flaps with a strong solution of Condy's fluid, say ℥ i. to ℥ x. of water; apply strapping, and cover the wound with a small piece of lint, wetted with a lotion of ʒ ij. carbolic acid, ʒ ij. liq. potassæ, to six ounces of water; cover with a piece of gutta percha tissue, and continue until healed, surrounding the edges or flaps with a little pad of carded lint. The carded wool, or marine lint, made by Mr. Westhropp, of the Falcon Works, London, is an excellent material for the purpose. (Dr. J. S. Walker, p. 183.)

M. Guerin's Method of Dressing Wounds.—During the late siege of Paris, the cases of important operations, such as amputations and excisions, as a rule did very badly. Having been struck with the experiments by Pasteur, Tindall, and others, illustrative of the dust and germ theory of disease, he bethought him of entirely encasing all surgical wounds with a thick coating of cotton-wool, with the view of filtering the air which had access to the part. Take the dressing of an amputation for example: he wrapped the stump round and round with successive layers of cotton-wool; a liberal use of the substance must be made, and several yards should successively disappear round the limb, and it should be carried up the limb to the body, and in the case of the thigh, as far as the waist. By this means all the approaches to the wound are carefully guarded in every direction. The whole should be gently supported by cotton bandages. This dressing is to be permanent for twenty-five or thirty days. On

removing it a healthy granulating surface is discovered, and half a wineglassful of healthy pus is found within the folds of the cotton. This plan turned out very successful, the mortality after surgical operations from gunshot wounds being considerably lessened. (M. Guerin, p. 184.)

AFFECTIONS OF THE SKIN, ETC.

BURNS.—*Oakum as a Dressing for.*—Common oakum, soaked in water, laid on the surface of a burn, and retained in position by a bandage loosely applied, forms a most excellent dressing. It prevents all smell, and promotes healthy and rapid cicatrisation. (Dr. H. L. Snow, p. 259.)

Hydride of Amyl.—Hydride of amyl is a perfectly bland unirritating liquid, so volatile that it evaporates very rapidly, indeed it boils with the heat of the hand. It is an excellent solvent for oils and fats of every description, which by its means may be distributed very evenly over the surface of a burn or scald, so as to exclude the air. For this purpose spermaceti, cut into fine shavings, should be added to the hydride until it is saturated, when a sixth part of olive oil should be added. When this solution is poured freely over a burn or scald, the cooling which occurs immediately relieves the pain, and a pellicle or false skin of fatty matter is left upon the wound. If a little cotton-wool is loosely pressed upon the part, the rapidity of evaporation is subdued, and the dressing is more effective. The wool may be left upon the wound, more solution poured over it, and a bandage applied, under which dressing the burn will heal, if not very large. (Dr. B. W. Richardson, p. 381.)

HERPES ZOSTER.—The grand points in the treatment of herpes zoster are to exclude the external air by the application of flexile collodion constantly re-applied, and to prevent the neuralgia rising to any great height of severity by the subcutaneous injection of $\frac{1}{6}$ grain of morphia as often as required. (Dr. F. E. Anstie, p. 240.)

GANGLION AT THE WRIST.—Most of these cases can be treated successfully by subcutaneous incision, followed by squeezing out the contents into the surrounding tissues, and the application of a compress and bandage. Other plans of treatment are the following:—The joint may be flexed to the utmost and firm pressure made on the tumour with the thumbs; if the tumour is not got rid of by this means, a thread should be passed through it, and pressure made upon it as before, so as to empty it through the punctures made by the thread. The two ends of the thread should be tied together, and retained

so until inflammation is set up, when the thread may be removed. A hare-lip pin may be passed into the cyst and made to puncture it in five or six places at the opposite side without puncturing the skin except at the point of entrance, pressure must then be applied as in the other plans. The cyst may be ruptured by pressure with the thumbs and then emptied of its contents by steady rotatory rubbing; the part should be painted with a strong solution of iodine and a pad and bandage applied. (Report on Practice of London Hospitals, p. 245.)

Forcible rupture by digital pressure, followed by continued pressure, combined with the use of a splint, have given the best results. In old and neglected cases, the ganglion, instead of being confined to the tissue between the tendon and the skin, extends around the tendon, and thus every movement of the tendon after the rupture prevents the adhesion of the walls of the sac, hence the necessity for a splint. (Mr. Vincent, p. 258.)

Never forcibly rupture, but evacuate subcutaneously, by first incising the sac freely with a tenotome, then squeezing out the contents of the sac, and firmly pressing the walls together with a compress and strap of plaster. Should it recur, pass a fine silk thread through it with an ordinary suture needle, and leave it for three or four days. Injection with iodine is not very satisfactory. (Mr. B. Hill, p. 257.)

Skin Grafting.—*An Instrument to Facilitate.*—Mr. Ferguson the instrument maker, has at the suggestion of a Student of St. Bartholomew's Hospital, brought out a most ingenious contrivance for facilitating skin grafting. It consists of a pair of curved scissors which are provided on their concave surface with bent forceps. These are controlled by a lever, which descends with the separation of the blades and rises when they are brought together. When the blades are separated the forceps descend, seize a small portion of skin, which they withdraw above the blades as they close. (p. 259.)

Suppurating Sores and Wounds.—*Mode of Applying Iodine to.*—Iodine dissolves readily in hydride of amyl, and when this solution is applied to the skin, the volatile hydride passes off and leaves the iodine behind, stranded on the part in most equal form of distribution. This application is of singular utility in cases of hard open sores, where it is desired to apply iodine evenly and deeply. Thus, in cases of open strumous glandular disease, the solution plays an important part as a means of cure, and the same in chronic indolent bubo. In bad sloughing fœted ulcerative and suppurative wounds, and in cancer, no solution is so simple, painless, and

effective. In these last-named cases it exerts more than a curative influence, it destroys decomposing organic products. The solution may be gently poured over the parts. There is no necessity either for cotton-wool or brush. (Dr. B. W. Richardson, p. 378.)

Vaccination.—The greatest care should be taken that pure lymph, without the least speck of blood or any foreign matter, is used. If this old and well founded rule is carried out, and only children which are healthy both in themselves and in their family history are vaccinated from, there is not the least reason to suppose that any constitutional disease like syphilis can be transmitted. (Ed. of Lancet, p. 264.)

VENEREAL AFFECTIONS.

Gonorrhœa.—There is no difficulty in running a stream of water through the urethra without inoculating its deeper portion. This is a better mode of applying injections than the syringe. The water used should be medicated with half a grain of permanganate of potash to the ounce of water. The amount may be increased up to two or three grains, but beyond this it causes pain and seems to take less effect on the discharge. Syphon power may be used, the vessel from which the injection flows being elevated considerably above the patient. This causes a steady and uninterrupted flow. A piece of small vulcanised india-rubber tube may be used for insertion into the urethra. An ordinary india-rubber enema ball and tube may be used for the same purpose, the ivory end being replaced by a glass cylinder. This renders the process very easy. (Mr. T. Windsor, p. 279.)

Modification of Disease by Constitution.—The most intensely specific diseases, such as syphilis, are always modified by the peculiarities of the patient's constitution. The course of syphilis in a gouty and in a scrofulous man, for instance, is quite different. It is among the first necessities for success in practice that, in the total phenomena of a disease observed in any patient, we should be able to estimate what belongs to the disease and what to the man. (Sir J. Paget, Lancet, June 10, 1871, p. 776.)

Syphilis.—Experience in a regimental hospital, where the opportunities of watching cases for almost any length of time are very great, has convinced the author that in the infecting form of sore (whether soft or hard) there is but one safe practice, and that is, the continued administration of mercury in one form or another. It does not always prevent the occurrence of secondary symptoms, but it certainly postpones their

occurrence and mitigates their severity. In the year 1864 the Lords Commissioners of the Admiralty appointed a committee to enquire into this subject thoroughly. The result of their investigations was in substance the same as the above opinion of Mr. Venning. As to the form of mercury most suitable for administration twenty-six witnesses were examined. Eleven gave the preference to blue pill, six to inunction, three to fumigation, two to mercury and chalk, two to Plummer's pill, one to bichloride of mercury, and one to protiodide. As a rule blue pill is well borne by the patient, and is a clean and convenient preparation. It is a good plan to combine it with a tonic, say three grains of blue pill to two of quinine. (Mr. E. Venning, p. 272.)

AFFECTIONS OF THE EYE.

Closure of Wounds of the Cornea by Suture.—Whenever a wound of the sclerotic or cornea gapes a suture should be applied, unless there is some special contraindication. Chloroform should be generally employed, and the thread should be very delicate. Dr. Williams, an American surgeon, who had up to 1867 applied a suture to the corneal flap in upwards of 100 cases of cataract, uses a single strand of the finest silk, and a fine needle a quarter of an inch long, with a flat cutting point. The needle is held by strong forceps and passed as near as possible to the edges of the wound, which are held by very delicate forceps. In a case of wound of the ball of the eye union at once followed the application of a suture, though the edges of the wound had been separated for many weeks. (Mr. T. Windsor, p. 235.)

Extraction of Cataract.—The section must be purely corneal, not made where the cornea merges into sclerotic, for at this part union takes place with far more difficulty. The usual cause of failure in this operation is the making the section too small for easy delivery of the lens. The easy extraction of a cataractous lens, like the easy extraction of a stone in the bladder, depends upon a clean cut and a fair-sized opening; while a difficult and forcible removal is almost as certain to end disastrously. (Mr. J. Hogg, p. 226.)

Granular Ophthalmia.—*Local Use of Quinine.*—Some cases of granular conjunctiva with corneal opacity are greatly benefitted by placing a small portion of quinine within the lower eyelid, on the point of a penknife. The operation is followed by severe smarting which continues for ten or fifteen minutes—but this is not invariably the case. In all cases considerable purulent discharge takes place, with shrinking

of the granulations, and clearing up of the surface of the cornea. The intolerance of light ceases rapidly in all cases. Dilatation of the pupils is produced in from twelve to twenty-four hours after the first application, but they contract at once if exposed to a strong light. (Mr. C. Bader, p. 233.)

Opacities of the Cornea.—If other means, including the pure turpentine oil, are found insufficient, touch the eye with a solution of one part of croton oil in seven of olive oil. This should be considered the standard solution for the purpose, but, on the first application, this solution should be diluted with an equal quantity of olive oil. Severe pain, great redness of the eye, with much lachrymation, usually come on in a few minutes. (Mr. T. Windsor, p. 235.)

MIDWIFERY AND THE DISEASES OF WOMEN.

Application of Strong Nitric Acid to the Interior of the Uterus.—Obstetric surgeons are becoming bold, for Dr. Atthill relates a case of sub-involution of the uterus following labour, and attended with menorrhagia, in which, after dilatation of the os and cervix, he cauterized the entire of the inner surface of the uterus freely with strong nitric acid. *No pain was caused.* The case was discharged cured in a short time. (Dr. L. Atthill, p. 334.)

Cancer of the Uterus.—*Destruction by Bromine.*—Dr. Wynn Williams says, "My own opinion is most decided that cancerous growths situated in the lips or neck of the womb can be successfully eradicated when confined to this part only, and previous to the commencement of ulceration. A solution of bromine in spirits of wine (12 drops in a drachm) is to be injected into the substance of the abnormal structure by means of a fine syringe. As, however, it is almost impossible to force the point of a fine needle into the more solid fibroid growths, a small trocar and canula, to which a glass syringe can be attached, should in this case be used. The point of the needle or canula should be withdrawn a little before injecting, or the fluid cannot be forced into the interior of the tumour. Before injecting the bromine the neighbouring parts should be protected by cotton-wool saturated with a solution of carbonate of soda. A slough will result, and separate in a week or so, and the process may be repeated in the same or another part of the tumour, although it is not necessary to wait until the first slough has separated. Instead of injecting the solution, it may be applied on cotton-wool directly to the os. The wool should be saturated with the solution and contained in a vulcanite cup. A

slough results, as in the other case, but it is shallower and more extensive. No evil can possibly ensue, supposing the tumour thus treated should not be malignant, as it is certainly as ready a mode of getting rid of it as any other. A number of cases have been treated in this way without any return of the disease. (Dr. A. W. Williams, p. 361.)

Decapitation in Shoulder Presentations.—In cases of shoulder presentation, when either the pelvis of the mother is small or distorted, or the shoulder is wedged in the pelvis, the waters long discharged, and the uterus tightly moulded upon the body of the child, sometimes it is impossible to deliver by version, or only at such risk to the mother as to be unjustifiable. In these cases decapitation of the child is in most cases preferable to evisceration. It is a much older operation than evisceration, being described by Celsus and several later writers. To perform it it is only necessary to pass a piece of strong cord round the neck of the fœtus by means of an elastic catheter, and then to saw through the neck with the string. The string should be attached to the point of the catheter, which must be pushed forward, whilst the curved stilet is retained. The curve of the stilet will cause the catheter to curl round the neck. The ends of the cord should be crossed and passed through a speculum, so as to protect the soft parts of the mother; or if no speculum is at hand, two spoons may be used for the same purpose. (Dr. G. H. Kidd, p. 303.)

Dilatation of the Os and Cervix Uteri.—As is well known to those who have once tried the dilatation of the cervix uteri for the purpose of examining by touch the uterine cavity, after the withdrawal of the tent or tents it is found that the os internum has resisted free dilatation. Although the finger can be passed up the cervix, it cannot pass the os internum. Instead of using another tent, the best plan is now to introduce the smallest-sized Barnes' dilator, and dilate with air or water. The dilatation is effected more safely and rapidly. This diagnostic proceeding is always called for if in an otherwise healthy woman we cannot detect by careful examination any disease to account for persistent menorrhagia. After the dilatation the finger cannot be passed over the whole internal uterine surface without drawing the uterus down by means of a vulsellum forceps attached to the anterior lip, and at the same time pressing the organ down into the pelvis from above (this should be done by an assistant). Sea-tangle tents are preferable to sponge; they are more cleanly, and dilate the passage more uniformly. (Dr. L. Atthill, p. 326.)

Dilatation of the Os and Cervix Uteri in Supposed Polypus.—There is no danger in dilating the os externum, but if it is proposed to dilate the whole cervix, including the os internum, the greatest circumspection is required. Metritis, ovaritis, perimetritis, are frequent results of this exploration, and death itself may be a consequence. It is probable that forcible dilatation of the cervix uteri in cases where the upshot shows that it was required, is not nearly so liable to be followed by untoward accidents as when it is done in cases which turn out not to have required it. (Dr. J. M. Duncan, p. 351.)

Duck-Bill Speculum.—*How to Use.*—The thighs are flexed at right angles with the pelvis, the patient lying in a semi-prone position on her left side, her left hand being drawn backwards under her, and kept in that position; the chest rotated forward, bringing the sternum very nearly in contact with the table or couch, the head resting on the parietal bone; the head must not be flexed on the sternum, nor the right shoulder elevated; the patient is thus rolled over on the front, making it a left lateral semi-prone position. The nurse or assistant at her back pulls up the right side of the nates with her left hand, whilst the surgeon introduces the speculum, elevates the perineum, and gives the instrument into the hand of an assistant, who holds it firmly in the desired position. (Dr. M. Sims, p. 322.)

Fibrous Tumour of the Uterus.—*Incision of the Os and Cervix Uteri.*—This operation which was first brought into prominent notice by Mr. Baker Brown is founded on the theory that division of the os and cervix uteri permits the fibres of the body of the uterus to contract upon the contained tumour, and thereby to compress the vessels and prevent hemorrhage. Whether this be the true explanation or not, one thing is quite certain, that the operation is often followed by good results, and that in the case of very large tumours which are contained within the uterus, and when the cervix is thinned and spread over them, the operation is fully justified. (Dr. L. Atthill, p. 359.)

[This operation was recommended by Mr. Brown especially to arrest hemorrhage from these tumours, which it does most effectually and at once. Eds.]

Internal Medicines in Fibrous Tumour of the Uterus.—Medicines without number have been given for the purpose of producing absorption of fibrous tumours of the uterus. The most prominent among these are the bromides. After trying them fully and freely I have come to the conclusion that they are all perfectly useless. (Dr. L. Atthill, p. 358.)

Hemorrhage from a Fibrous Tumour in the Uterus.—Hemorrhage from a fibrous tumour within the uterus may be arrested by injecting the uterine cavity with a drachm of the liq. ferri perchloridi. The cervix should first be dilated. Of course this is only a palliative and temporary treatment. (Dr. L. Atthill, p. 360.)

HYSTERIA.—*A Test for.*—In every case of hysteria the commencement of the affection is marked by a special symptom, insensibility of the epiglottis. To determine the presence or absence of this symptom, introduce gently the forefinger into the mouth so as not to frighten the patient, and place it on the base of the tongue. It will be found that the epiglottis may be touched, displaced, and scratched with the nail, without producing the least regurgitation. When this symptom exists there will be found a congestion of one or both ovaries, usually the left. Out of 26,000 female patients this symptom existed in every case of hysteria—very numerous out of such a large number of cases. (Dr. Chairon, p. 80.)

INJURY TO THE SPHINCTER ANI DURING PARTURITION.—In cases of laceration of the perineum with injury to the sphincter ani, the muscle merely being injured, not completely torn across, there is a tendency to looseness of the bowels, almost amounting to diarrhœa, and a source of great annoyance to a person of any refinement. It can only be cured by restoration of the perineum by operation. (Dr. J. M. Duncan, p. 287.)

MECHANICAL DYSMENORRHŒA.—In cases of pin-hole os uteri with dysmenorrhœa, dilatation with sea-tangle tents should be performed, for nothing but this or dividing the os and cervix uteri with a hysterotome will suffice for the cure. Even supposing the latter operation is necessary, it is nearly always requisite to dilate the cervix with a small tent to allow of the introduction of the instrument. If, owing to anteversion of the uterus, there is difficulty in introducing the tent, a duck-bill speculum should be used to expose the os, which should be drawn down with a fine hook. If after trial of simple dilatation it is thought advisable to incise the constricted parts, we have the choice of several methods, the use of Dr. Savage's hysterotome is perhaps the best. It is passed through the os internum, and then by means of a screw in the handle its two fine concealed blades are made to protrude to a limited extent, which is increased when the os internum is incised. As the instrument is withdrawn the extent of incision is still further increased, or it may be completed by means of a curved scissors. It is upon the whole safer to divide the vaginal part of the os with the scissors. As there is often a tendency

to unite, it is a good plan to introduce a thin roll of lead. (Dr. L. Atthill, p. 330.)

MENORRHAGIA FROM SUB-INVOLUTION OF THE UTERUS AFTER ABORTION.—Supposing a patient subject to constant menorrhagia following abortion: the first thing must be to dilate the os and cervix uteri by one or more sea-tangle tents, and then having satisfied oneself that the hemorrhage does not depend upon the presence of polypus uteri, or disease of the mucous lining of the uterus, to measure the cavity of the organ by means of a Simpson's sound. If found enlarged, the hemorrhage depends upon sub-involution of the uterus. This must be treated on the principle of stimulating the womb to take on a healthy action and tone. No plan is better than passing into the uterine cavity ten grains of nitrate of silver, and leaving it to dissolve there. This must be done by means of Simpson's "uterine porte caustique," which consists of a hollow silver tube closely resembling a sound, and containing a stilette. Considerable pain is generally produced, which lasts five or six hours, but no further unpleasant results follow. This plan is most efficacious, and has not in any case been known to produce any serious symptoms. (Dr. L. Atthill, p. 335.)

MIDWIFERY FORCEPS.—*The Mode of Introducing.*—Dr. Cappie, of Edinburgh, considers that in introducing the midwifery forceps, the task of introducing, guarding, and directing should be trusted entirely to the fingers of the right hand, and that, in the first stage of introduction, the left, crossed over the right wrist, should have little more to do than to support the shank or handle of the instrument. The point should be carried forward almost entirely by the index finger of the right hand, introduced within the fenestrum, and just catching on the inner edge of the rim. He says, "those who have never tried this plan will be surprised to find how easily it can be urged onwards, and how safely the proper direction can be given. When the finger has pushed it onwards as far as it can, the left hand becomes the propelling force. The forefinger, however, still acts at any convenient point as a fulcrum, to assist in giving the proper direction to the blade, while the hand gives the rotating and forward movement. (Dr. J. Cappie, p. 294.)

MOTHERS' MARKS.—*To Remove.*—Dr. Berwick, of Sunderland, recommends the use of strong glacial carbolic acid for the removal of these disfigurations. He effected the removal of a large, hairy, corrugated, dark-coloured mark by its means. If of large size a part only should be operated on at a time, and to this part the acid should be applied twice a

day. A scale or crust will form, which in time will drop off, leaving the skin beneath perfectly free from blemish. (Dr. G. Berwick, p. 375.)

Ovariotomy.—*Treatment of the Pedicle.*—The pedicle should be treated after the following fashion:—If the pedicle were long, narrow, and could be easily brought outside without traction upon the uterus or making any undue pressure upon the wound at the lower part of the belly, I should most certainly use the clamp; if, on the contrary, the pedicle were short, and I feared dragging or too much displacement of the uterus, and if it were not too voluminous, then I should transfix it with a firm ligature crossed so that it could be tied on one side only, cut the ligature close, and drop the stump into the pelvis. In cases where the pedicle could neither be safely brought out with the clamp nor ligatured for fear of vessels retracting or tissues shrivelling up; or where the pedicle was very short, vascular, and fleshy, and I feared the ligature might cut it; or where it was very close to the uterus, I should secure it with a clamp (moderately crushing it), divide it, secure each vessel with a ligature cut short, and apply the actual cautery to the rest of the stump, waiting a few minutes to see if any oozing of blood took place before dropping it into the pelvis. If the pedicle were very voluminous, the cyst having a very broad attachment to the uterus, so that I could not use the clamp to hold and crush it whilst I applied the cautery, I should divide it with a pair of blunt scissors slowly, taking up each vessel as I went on, and apply the cautery to the remaining portion of the stump. (Dr. D. L. Roberts, p. 338.)

Polypus Uteri.—*Hemorrhage as a Symptom.*—Although undoubtedly hemorrhage is the great sign of polypus uteri, its constancy as a symptom has been very much exaggerated. Among forty-one mucous polypi, nineteen, or about one-half, caused no extraordinary loss of blood whatever; while in several there was less than ordinary loss, and in some none of any kind. All fibrous polypi, however, at one period or other of their growth cause loss of blood. Fibrous polypi are not so common as mucous. (Dr. J. M. Duncan, p. 350.)

Retention of the Secundines after Abortion.—At the early and the latter months of pregnancy there is comparatively little difficulty with the secundines, because, during the first two months, the aborted ovum is usually cast off entire; and during the last three months, if the placenta does not come away after the expulsion of the child, the uterus is sufficiently large to admit of manual extraction of it. But during the third, fourth, or fifth months, the womb

is so small and the vagina so undilated, that the removal of retained secundines can only be accomplished with the greatest difficulty. It is the best plan to place the patient under the influence of chloroform, and then carefully and gradually to introduce the whole hand into the vagina. If the os uteri is closed, it must be gradually dilated by passing first the forefinger into the orifice and then the second, with as little force as possible. The introduction of the fingers is generally effected easily. The placenta must then be removed with the fingers. When the os will only admit one finger, and the last phalanx cannot be passed much beyond the os internum, just reaching the retained placenta, a modified form of the ordinary ovum forceps (and invented for this purpose by Dr. Swayne) should be passed over the forefinger of the left hand, which fits within their hollowed inner surface, and when the os internum is reached, it may be enlarged by opening the blades, by means of a screw in the handle. The retained placenta can then be removed by the forceps, guided by the finger. (Dr. J. G. Swayne, p. 307.)

Sickness of Pregnancy.—The hydrated phosphate of lime, given in doses of from three to ten grains, three times a day, generally relieves the sickness of pregnancy. It should be simply suspended in water. The possible explanation is, that the requirements of the child in the production of bone, robs the nervous system of the mother of the phosphates necessary to its own health. (Mr. M. Johnson, p. 291.)

Tedious Labours.—*The Management of.*—By attending to certain rules, the result of his experience, Dr. Hamilton, of Falkirk, has been so successful as to have 731 children born alive successively, the 732nd being still-born. He never interferes with the first half of labour, however prolonged, but allows the os to be dilated naturally by the bag of membranes. The recommendation of the late Professor Hamilton, not to allow the first stage to last more than from twelve to fourteen hours, he considers bad advice. Supposing the membranes to be ruptured, and a rigid os uteri nipped between the child's head and the pubes, or the promontory of the sacrum, manual dilatation should always be resorted to, or the os will become swollen and inflamed, and so present still further obstruction to delivery. For this purpose, if necessary, the whole hand should be introduced, and the os resolutely pushed up over the head. The second stage of labour should never be allowed to go on more than two hours without the application of the forceps. In Dr. Hamilton's practice he applied them in one case in eight. In special cases we must not wait so long as two hours. The

forceps should not be greased before introduction, as this tends to make them slip; on the contrary, it is a good plan to smear them on the inside with India-rubber paste, which soon dries, and enables them to lay firmer hold of the scalp than does the bare metal. Ergot should by all means be avoided. Dr. H. believes that the child's life is frequently lost in cases where everything is fair, the head well down in the pelvis, but the case lingering and protracted. (Dr. G. Hamilton, p. 281.)

MISCELLANEA.

Anæsthetic Ether (Dr. Richardson's) for Local Anæsthesia.—This ether is composed of one part of hydride of amyl to four of rectified ether. Pure hydride of amyl is so volatile that it freezes too rapidly if used pure. The superficial parts become instantly frozen, and act as a non-conductor, preventing the deeper structures subsequently becoming frozen. (Dr. B. W. Richardson, p. 377.)

Chloral Hydrate.—*Toxical Effects of.*—Dr. N. R. Smith, of Maryland, relates several cases, showing apparently that the long-continued use of chloral is liable to be followed by certain toxical effects. There may be a troublesome disquamation of cuticle around the nails, accompanied with superficial ulceration. It, in fact, produces somewhat similar effects to ergot when administered continuously, as the effects on the fingers justify us in believing. In large doses, say of thirty grains, its administration is not free from danger, especially if one dose is administered before the effects of a previous one are quite gone. (Dr. N. R. Smith, Lancet, Sept. 20, p. 466.)

Chloral Hydrate and Opium.—Chloral has this great advantage over opium as a hypnotic, that it does not lose its potency by repetition; as, after the lapse of a few weeks, during which time it has been regularly taken, the same dose will produce effects as marked and as happy as when first administered. (Dr. N. G. Mercer, p. 398.)

Chloroform in Surgery.—The more I have used chloroform the less has my confidence become in its innocuousness. When I compare the results of my operations performed before anæsthetics were employed, with those performed during the last twenty-two years by the aid of chloroform, I am satisfied that unpleasant secondary results were less frequent during the first period than they have been under the use of that agent. I allude to secondary hemorrhage, pyæmia, erysipelas, and hospital gangrene. (Dr. N. R. Smith, Lancet, Sept. 20, p. 467.)

Chloroform Accidents.—Our aim should always be to give chloroform equally mixed, and in the smallest proportion needed for the purpose, so that if the effect becomes greater than is wanted, there may be as little as possible of the agent in the chest requiring to be got rid of. Clover's apparatus enables us to administer the vapour of three minims and a quarter of chloroform with every one hundred cubic inches of air. Should any laryngeal obstruction be threatened, the chin should at once be raised. An obstruction to the breathing sometimes occurs of a reflex kind—it is nothing but an act of swallowing imperfectly performed. Mr. Lister recommends the tongue to be drawn forward if laryngeal obstruction occurs, but Mr. Clover observes on this point: "I have met with several cases of partial obstruction of the larynx where the mere depressing or raising of the chin was sufficient to open or close the passage. In such cases, if I had tried to open the mouth by pushing down the chin, I should by that very means have caused total obstruction, and then, no doubt, dragging out the tongue would have been followed by a free inspiration, and probably louder and freer than would have been produced by the simple process of elevating the chin." (Mr. Clover, British Medical Journal, July 8, 1871, p. 33.)

Death from Chloroform.—Chloroform, ether, and other allied chemicals, cause death simply by poisoning the system by carbon, as do drowning and strangling. But in the former case the carbon is actively introduced; in the latter, the poisoning is passive, owing to deprivation of oxygen. In threatened death from chloroform, our endeavour should be directed to supply oxygenated blood to the brain by lowering the head, so that what arterial blood is contained in the arteries may gravitate into the head. If the brain respond, all is safe; the heart resumes its labours, and the lungs answer to the call. If, however, we do not succeed in rousing the brain, the case is hopeless. Dragging out the tongue is no use—and artificial respiration is only loss of time. Let the head hang well down, and colour will return to the lips, and the functions of life will be resumed. (Mr. J. D. Brown, British Medical Journal, July 22, 1871, p. 93.)

Colour Tests for Blood. — Prepare some fresh tincture of guaiacum by putting a few pieces of pure and unoxidized guaiacum resin into a small bottle and filling it up with alcohol —after shaking a minute or two it is fit for use. The stain must be moistened with this, and if this is followed by a negative result (many substances would produce a blue colour) we must add a few drops of antozonized ether, when,

if blood be present, oxidation and blueing of the guaiacum will follow. Robbins's ozonic ether is the best for this purpose. Blood produces no change in the colour of guaiacum except in the presence of antozone. A liquid has become antozonized when it is changed by absorption of oxygen from the air. All essential oils take on this change rapidly, and they then acquire the property of combining in all proportions with alcohol, and a little essential oil diluted with alcohol will do very well instead of the ozonized ether. By variations of this test mucus, saliva, and pus may be detected. It appears probable that this test will prove of service in medical cases—in testing for blood in matters vomited or expectorated. (Dr. John Day, of Victoria, Edin. Med. Jour., May 1871, p. 1049.)

Cotton-Wool Respirators to Prevent Infection.—Prof. Tyndall recommends the use of cotton wool respirators for infectious places. He believes they would if properly constructed be perfectly efficacious in preventing the catching of a contagious disease. (Prof. Tyndall, p. 25.)

Disinfection.—*To Purify a Sick Room or Ward.*—No substance equals iodine for this purpose; but its successful use requires certain conditions, viz., length of time to act and complete diffusion through the atmosphere. The room should be completely stripped of all furniture, the walls rubbed down, and the floors well swept, scrubbed, and dried. A solution of iodine in hydride of amyl of the strength of twenty grains to the ounce should then be distributed in spray by means of a glass spray-producer, from different positions in the room. The room should have all its windows and doors closed before the iodine is distributed, and the quantity of solution used should be about one ounce of the solution to four square feet of space. After this the room should be kept closed for twenty-four-hours at least, during which time the iodine, deposited at first in the finest layer on the floors, ceiling, and walls, slowly volatilizes and coming into contact with the organic matter destroys it rapidly. A light must not be taken into the room, or the amyl hydride vapour will take fire. (Dr. B. W. Richardson, p. 379.)

Hydramyl as a General Anæsthetic.—If hydride of amyl is carefully administered, insensibility to pain may be produced without loss of consciousness. The fluid however is so extremely volatile that it evaporates too rapidly. If to the materials for making bichloride of methylene (alcohol, chloroform, and pure zinc) about eight times the volume of hydramyl, is added a compound fluid distils over consisting of one part of bichloride of methylene to nine of hydramyl. This fluid is not quite so volatile as pure hydramyl, and yet

possesses the same properties as an anæsthetic. In the administration of this fluid, a properly constructed inhaler should be used, and the first symptoms of involuntary movement carefully watched. This is generally twitching of the eyelids or slight involuntary movements of the hand. The inhalation should then be given up, as, on account of the insolubility of the fluid in the blood its full effects are not felt for almost 10 seconds after the first appearance of involuntary movement. This slight interval prevents hurry—for the patient is quite conscious, and can be told to alter his position slightly, or open his mouth widely for the extraction of a tooth. There is complete insensibility to pain combined with perfect consciousness. (Dr. B. W. Richardson, p. 387.)

Organic Bromides.—Bromine may be considered as a medicine which acts primarily on the sympathetic system and as a modifier of vascular tension. It produces to a certain extent decreased sensibility of the nerves which govern common sensibility. We may administer bromine in combination with any other substance with which it will combine chemically, and thus procure valuable remedial agents having the action of their two constituents. Bromide of quinine, bromide of morphine, and bromide of strychnine are perhaps the most valuable of these. Two of these may be combined, thus bromide of quinine and bromide of morphine may be combined in syrup, of which a fluid drachm contains a grain of the quinine, and an eighth of a grain of morphine. Speaking generally the bromide in these salts is eliminative and sedative, and it is the result of experience that the bromide of quinine can be administered when quinine itself cannot be tolerated. It is certain also that the bromide favours the sedative action of morphia, while it at the same time allays the astringency which morphia induces. The bromides of quinine and morphine in combination constitute a most valuable medicine in the four following classes of cases, viz., neuralgic fever, cerebral irritation, diabetic phthisis, and extreme acute attacks of intermittent pulse, the result of organic nervous shock. In acute neuralgia it effectually removes the pain without producing any unpleasant symptoms. It calms pain without inducing deep narcotism, it interferes little with the secretions, it rarely causes nausea, and it interferes little with the appetite. (Dr. B. W. Richardson, p. 393.)

Strychnia Poisoning.—*Calabar Bean and Chloroform.*—Dr. John White, of Glasgow, relates a case of strychnia poisoning in which $3\frac{1}{3}$ grains of strychnia had been taken with intent to commit suicide. The treatment adopted was the inhala-

tion of chloroform to check the extreme severity of the spasms, and the internal administration of calabar bean. The chloroform acted very well and was used freely, but the calabar bean was only given twice in doses of half a grain of the extract. No treatment can be conceived more suitable than this, as calabar bean directly and powerfully diminishes the reflex excitability of the spinal cord, so much so that it is difficult by galvanism to excite any reflex movements in a frog poisoned by it. In this mode of action it is far superior to curare, belladonna, and tobacco. (Dr. White, Glasgow Medical Journal, August 1871, p. 488.)

Vulcanite Urinometers.—Urinometers made of vulcanite are cheaper and more durable than those made either of glass or metal. They, moreover, take the specific gravity more rapidly, and are unacted upon by all acids or alkalies, and by no liquid except bisulphide of carbon. They may be obtained from Messrs. Blaise, of St. James's Street, London. (p. 401.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—OBSERVATIONS ON SCARLET FEVER, ESPECIALLY WITH REFERENCE TO ITS EPIDEMIC CHARACTER.

By Dr. A. W. Barclay, Physician to St. George's Hospital.

[The miasm of scarlet fever, though yet untraced, must be some sensible portion of matter. It may be infinitesimally small, but matter it must be in some state of change, and, like other matter in a state of change, liable to excite similar change in other portions of matter capable of being acted on. The miasm is no doubt an organic compound, not gaseous, and not a vapour, but capable of suspension in air, or in the aqueous vapour combined with it.]

For my own part, I can see no probability of truth in that which would associate its propagation with living organisms. Even if all were granted that has been assumed in regard to the influence of germs floating in the atmosphere on the healing process in breaches of surface, it by no means follows that the metamorphosis of tissue throughout every part of the body should be influenced by the presence of such germs on the outer surface of unbroken tissue, whether applied to the skin or to the mucous membrane, and is, in fact, opposed to the actual knowledge we possess of the action of epiphytes on either.

But I would say farther, that the analogy which has been supposed to exist between suppuration and fermentation or putrefaction fails in one most material point, inasmuch as the living organisms developed from germs are found, and found invariably, in the putrefactive process; and by the introduction of the germ in certain chemical conditions of matter, fermentation or putrefaction may be excited at will; while no such evidence has been adduced with reference to suppuration. On the other hand, it is well known that the course and pro-

gress of the healing process is invariably influenced by the presence of dead animal or vegetable matter; that, in broad outline, the law by which suppuration is excited by the dead organic thread of a silk ligature, and is not excited by a silver suture, applies to the reunion of tissue under all circumstances whatever. Consequently, if it be thoroughly established that chemically pure air will not excite suppuration, while air loaded with ordinary dust, the motes that float in the sunbeam, will as certainly do so, the explanation will be found in the law which regulates the action of dead organic matter on divided tissue in the living body.

Another hypothesis here meets us which cannot be so readily disposed of. It is alleged, with some show of reason, that the exudation from the surface of the skin, and more especially the cuticle, is the main source of infection. And the theory is of importance, because upon it has been based the suggestion that the spread of the disease may be to a great extent arrested by careful inunction and ablution of the skin. I suspect that this theory, whether true or false, has originated in an error very prevalent of late years in the writings of medical men, itself the offspring of a wrong use of terms. No expression is more common than fever poison. Persons are said to be poisoned by impure air or impure water. The poison is said to be got rid of or eliminated by the skin or the bowels, as the one or the other happens to be the organ chiefly affected by the disease. In such expressions there is a lamentable want of that precision of language and of thought which is absolutely essential to correct reasoning.

The term "poison" has been unfortunately suggested by the circumstance, that all the symptoms take their origin in the introduction into the system of something injurious from without; the facts which have been ascertained, and the theories which have been adopted with reference to ordinary poisons, being as a consequence applied to the so-called fever poisons. In truth, there is scarcely any analogy between them; and the differences in their origin and mode of action are so great, that it would be more consistent with reason to believe that what was true of the one class would be false when applied to the other. In tracing the history of such cases, the first difference that strikes us is the infinitesimal quantity in which miasm acts on the body; and with reference to smallpox, at all events, it may be asserted that in the practice of inoculation the amount of matter introduced under the skin bears no relation to the intensity of the symptoms resulting from it. Poisons, on the other hand, act only in sensible quantities, and the symptoms bear a direct relation to the amount introduced. Next we find in the one class a period of

incubation during which no signs of poisoning are shown, whereas no such condition is ever seen in the other. Lastly, from the body of the individual suffering from miasmatic disease an immense quantity of fresh miasm is constantly being given out; but we know that a person who has been poisoned cannot possibly discharge from his system more poison than he has imbibed.

Putting false analogies aside, we may, however, inquire whether there is any reason for assuming that the skin is more likely to be the medium through which the miasm of scarlatina is propagated than the mucous membrane. The two circumstances which, to my mind, offer the strongest presumption in favour of this view are—first, the much greater extent to which the skin is involved in the course of the disease; and secondly, the late period at which infection may be caught from bedding, clothing, &c. In smallpox we have the strongest possible evidence that the most direct element in the propagation of the disease is the fluid contained in the pustules on the skin; but, at the same time, I should very much doubt whether an unprotected person would be safe from infection in a confined apartment where a patient was lying in the early stage of fever before vesication had begun. In scarlatina the affection of the skin, though so different in kind, is yet so marked and so extensive, that if the cutaneous secretion take any part at all in the propagation of the disease, it cannot fail to be an important one. And whatever may be said of other modes of transmission, I think we cannot withhold our belief that the skin is in this disease, too, one of the most direct media of infection.

With reference to the period during which miasm will retain its infectious character, if articles of clothing, &c., be shut up in a box, I have no new facts to add; I would merely call attention to the circumstance, as indicating that it must be associated with some substance of a comparatively stable character. In all infectious diseases we know that chemical change will render the miasm innocuous. Vaccine lymph requires certain precautions to be adopted for its preservation; and if the miasm of scarlet fever were only carried about in the air or suspended in watery vapour, it would probably very soon lose its infectious properties through decomposition. It seems, therefore, not unreasonable to conclude, that when the disease has been propagated afresh after a long interval, the miasmatic character has been transferred to some material which was not liable to rapid decomposition; such a material, for example, as the dry cuticular surface, which, if preserved from moisture, will remain for a very long time without undergoing any great change in chemical character. At first sight it is perhaps

difficult to reconcile this condition of chemical stability with what has been already asserted of miasm generally, that it is matter in a state of change. On farther consideration, it appears that the two conditions have no closer relation to each other than is expressed in the statement that chemical stability is absolutely essential to the development of this vital change; that any substance rapidly undergoing chemical change is incapable of developing it; and that the chief means we possess of preventing its development is the employment, in the presence of miasm, of such reagents as tend to promote chemical decomposition, so that we have no better disinfectant than free oxygen.

But while admitting the great probability of scarlatinal miasm being propagated by means of the skin and its exudation, we have still to consider how this is brought about; whether as the result of some specific secretion, or as a consequence of general change affecting all the secretions alike; and farther, whether there is any evidence to show that the miasm attaches itself more to one secretion than to another. This question has been already in part answered. If, as we have asserted, there be no poison to eliminate, there can be no special secretion; the ordinary secretions, so far as disease does not arrest them altogether, will continue to go on. But their character, though unchanged to all appearance, must have received a certain impress from the universal action proceeding in every part of the body, and this is only known to us by its capability of exciting the same actions in another. So far as analogy can serve as a guide in reasoning on such matters, I think we should naturally conclude that one secretion was just as likely to have received this impress as another, and that the relative power of each in propagating the disease depends rather on secondary causes than on its direct relation to the miasm so propagated.

The elimination theory asserts, in opposition to this view, that during the existence of specific fever a poison is generated which must be eliminated or got rid of by some secernent action, and that the selection of the apparatus employed for this purpose is the chief cause of the symptoms by which they are recognised. Two questionable statements are involved in this proposition: viz., that there is a poison, and that it must be eliminated. To deny that there is a poison may seem paradoxical; but I merely wish to express that the word "poison" gives a totally erroneous idea of fever miasm. Water may have a striking resemblance to alcohol, because they are both colourless fluids; but they are essentially different in most respects, and to group them under a common name would be only to cause confusion. I need not again repeat the chief

points of difference; but in nothing is the distinction more marked than in the fact, that an infinitesimal quantity introduced will multiply itself almost illimitably in the individual suffering from the disease. If a man happen to have swallowed a certain quantity of opium or arsenic, we know that a foreign element is present in his constitution which must be got rid of sooner or later, the total amount remaining unchanged throughout. So long as any portion is retained, it does harm in an exact ratio to its quantity; in short, his life and health depend on the poison being eliminated. When, on the other hand, a man has imbibed a fever miasm, its quantity is infinitesimal, imperceptible; it acts as a disturbing cause in this minute quantity, and the measure of the disturbance is not regulated by the amount absorbed. After a time fresh miasm is generated; but the individual is no longer susceptible to its influence while it remains with him, and no perceptible change in his state is produced when it leaves him. To him the quantity given off is wholly immaterial, whether he serve as a focus of infection at the commencement of his attack or during the period of convalescence. To him it is much more important, be the attack mild or be it severe, that all the ordinary functions of life should go on without interruption; the arrest of any one of them, by exposure to cold or any other accidental circumstance, is of far more consequence than the possible amount of infectious miasm generated during his illness. For him there is no elimination.

If, then, we put aside the theoretical argument derived from the supposed necessity for the elimination of the poison, there remains, as it seems to me, no reason for believing that any specific secretion is concerned in the propagation of scarlet fever. No one who has watched its progress can doubt the extent to which the blood and tissues are involved, can doubt that though the skin-affection be one of the most prominent symptoms, it is by no means a primary element in the disease. Neither can we withhold our belief, that the action taking place in the skin is produced by and dependent on the more general affection. Were farther proof necessary that the skin only becomes a medium of infection in consequence of this circumstance, it seems to be afforded by the fact that in this disease the specific action which specially marks its place among miasmatic fevers affects the mucous membrane as well as the skin, producing ulceration of the throat in a large number of cases. The conclusion seems unavoidable that, so far as we at present know, any secretion, or more probably all the secretions, may contain matter in a state of change, which, when meeting with other portions of matter capable of undergoing the same series of changes, is liable to excite them,

except when prevented from doing so by some chemical or vital law. In fact, it seems not improbable that the secretions themselves, while not otherwise sensibly modified, have this special impress stamped upon them by their coming from an individual for the time suffering from the disease.

With reference to the means at our disposal for arresting the spread of the disease and aiding the recovery of those who are already under its influence, few words may suffice, as they are subjects now so well understood by the majority of well-educated medical men. We have assumed that the phenomena of abnormal vital action are excited by some portion of organic matter which is at the time of its entrance into the body in a state of change, and that the whole of the tissues may or do pass through a series of analogous changes, so that any effluvium or particle of matter given off is liable to excite the same changes in another person. Experience teaches that the miasm so given off is to a certain extent unstable, and may be rendered innocuous by chemical decomposition, and we therefore turn to chemistry for means to accomplish this end. The agents which have hitherto been chiefly employed may be divided into three classes—deodorisers, antiseptics, and disinfectants, according to their most prominent effects. Such terms are necessarily only relative, and do not imply that their action is limited to that which is exactly defined by the strict meaning of the word. Among deodorisers I would class such substances as Burnett's solution and the perchloride of iron, which at once deprive sewage and decaying animal matters of their fetid smell. They possess, in common with many other metallic salts, powerful chemical affinities, and will more completely neutralise free ammonia and decompose sulphuretted hydrogen than many other substances which are perhaps of more real value as disinfectants. At the same time it is probable that this power of decomposition extends beyond their more sensible effects to the organic miasms which accompany the fetid gases. It is not unlikely too that the chlorine they contain may be of some importance, though its action must be very different in such forms of combination from that with which its presence is usually associated in our minds. Antiseptics belong to a different class, and it is difficult to say to what extent they act as deodorisers, because the smell of the substance itself is usually so powerful. The one great representative of this class is carbolic acid in its various forms. Its power seems to consist in restraining organic matter from passing into that series of chemical changes to which it is liable as soon as it is removed from the controlling power of vital action, an effect which may be hastened or retarded by a variety of causes. If fever miasm consisted simply of matter undergoing putrefactive change, there could be no

question that carbolic acid would be the most certain disinfectant; and it is not unreasonable to believe, that if it possess the power to restrain putrefactive change, it may also arrest that other series of changes which converts the exudations from the sick into a means of propagating disease to the healthy. With this idea it has been very much employed of late, and possesses the advantage of being very diffusible through the atmosphere, and of indicating by its odour, which is not disagreeable to most persons, the extent to which this diffusion has taken place.

True disinfectants must consist of those substances which deprive the organic matter of its power to excite in any other living body that tendency to change which itself is for the time being subject to. The principle of their action would seem to be, that by chemical affinity the peculiar tendency of the organic particle is altered so as to bring it under the ordinary laws of decaying animal matter. Great heat and oxygen gas possess this power in the highest degree; and it is not improbable that all the substances employed as simple disinfectants owe their power to the fact that they tend to set oxygen free. Thorough ventilation of the sick-room acts not only by diluting the miasm and carrying it away as speedily as possible, but also by exposing it more freely to the action of the oxygen contained in the atmosphere. At the same time it would seem that oxygen liberated from some chemical combination acts most energetically in its nascent state, and hence chlorine gas and the permanganates are believed to destroy fever miasm more perfectly than any other agents. Unfortunately the vapour of chlorine is very irritating, and the permanganates are not diffusible through the atmosphere. The effect attributed to the burning of sulphur with the escape of sulphurous acid gas is probably explicable, though less directly, in the same way. To those who are inclined to hold the germ theory of disease it might offer some corroboration of their view, that this acid is especially destructive of the lower forms of vegetable life. The same remark will also apply to such temperatures as are employed for depriving articles of clothing of infection; for the very same degree of heat serves to destroy the life of the lower organisms, though they survive a temperature which the higher classes of animals cannot withstand.

In the earlier part of this paper I have endeavoured to show that the skin is not in all probability the only medium of conveying infection, but that its minute scales are very likely to retain it longer and convey it to a greater distance than any other exudation. It is therefore essential that, while every precaution is taken to preserve the purity of the atmosphere in the sick-room, special care should be taken that the clothing and bedding are thoroughly disinfected. The plan of inunction

which has been recently suggested would seem to have this one recommendation, that during the stage of desquamation the minute particles are more likely to be scattered about the room, and perhaps carried about in the atmosphere, if the skin be dry, than when it is moistened by an unguent; but it does not in the least degree render the adoption of other measures less necessary, both during the attack and after the patient has been removed from the room; the absolute prevention of the escape of a single particle of cuticle would not restrain the spread of the disease through other channels.

[Stimulants contribute to the end of sustaining the powers of life not only by their action as pure stimulants, but by their power of retarding the metamorphosis of tissue.]

Just as excess in stimulants in health will produce nausea, depression, and biliary disturbance, so in disease we must be careful to subordinate them to other forms of nutriment, and to have regard to the actual necessity for their administration. So long as food is taken well, and the digestion does not seem to fail; so long as the circulatory and the nervous systems do not point to the need of stimulus, it seems unwise to have recourse to them too early. Sooner or later, in larger or smaller quantities, they must find a place in almost every case of scarlatina; but in every instance it should be a matter of thought and consideration, and not one of mere routine practice, when stimulants are ordered. When the necessity is great, they must be given without stint, at least for a time; and the unexpected rallying of patients suffering under very severe forms of the disease is at once the justification of the treatment, and the explanation of the excessive mortality so often found among the poor.—*St. George's Hospital Reports*, 1870, *vol.* v., *p.* 167.

2.—ON SCARLET FEVER.

By Dr. E. Copeman, Norwich.

[There are three important weapons necessary to the treatment of scarlet fever, viz., fresh air, pure water, and good diet. The air dilutes and rarifies the poison; the water washes away impurities, and prepares the surface for the excretion of morbid matter; and the food fortifies the constitution so as to render it less obnoxious to the depressing effects of disease.]

One of the most common of the sequelæ of scarlet fever is dropsy, with albuminous urine; an untoward complication, and one which generally supervenes at a time when all idea of danger has been removed, or most probably in cases where no danger has been apprehended. It occurs more frequently after

mild cases; and early exposure to cold during convalescence appears to be the most frequent cause. Many years ago I published a short paper on Scarlatina in the Medical Gazette, vol. xxx. p. 96, chiefly with reference to the treatment of the dropsy which often supervenes. I there remarked, that 'this affection (dropsical effusion) is generally considered to be of an inflammatory character; and blood-letting and other antiphlogistic remedies are recommended for its removal. The general success of such measures is also established by the concurrent testimony of various authors; but during the late epidemic I had occasion to treat the disease in patients who had been so much debilitated by the previous attack of fever, that I had not the courage to further reduce the powers of the constitution.' I was then induced to make trial of iodine, which I prescribed in the form of Lugol's solution. 'Of this solution I ordered from five to ten drops for children, and from ten to twenty or twenty-five to adults, three times a day in water. In the first case in which it was used it rapidly effected a cure; in consequence of which I prescribed it in every succeeding case that presented itself, and with the same complete success. If iodine have not hitherto been employed for the cure of anasarcous swellings after scarlet fever, I believe I shall have the happiness to have introduced to the profession a safe and effectual remedy for a troublesome and obstinate affection. If others have employed it, I have the gratification to add my testimony to the good effects of a mode of practice not so well known or recommended as it seems to me to deserve.'

I have transcribed the above remarks because they are still applicable as regards my further experience of the treatment of scarlatinal dropsy. I have always used the same remedy (now in the form of the liq. potas. iodid. comp. of the London Pharmacopœia), and may now, as I did then, confidently affirm that it has invariably been attended with success. Sometimes it has for a while appeared to fail; but on increasing the dose the desired effect has been produced, and the patient has recovered. Many such cases have presented themselves at the Lind Infirmary for the diseases of children. All that have come under my care have been thus treated, and in no instance without a favourable result In private practice also many similar cases have been cured by it, where the disease had resisted other treatment, and led to my being called in consultation. The following is an instance of one of the most severe forms of the disease, in which this remedy was rapidly and entirely successful:

A child, aged 6 years, had been attacked with a very mild form of scarlet fever three weeks before. There had been but little eruption, and the child appeared to be rapidly recovering;

but after exposure to the air, having been allowed to go out, she was seized with anasarca, which gradually extended so as to become general. There was also effusion into the cavities. The urine was scanty and albuminous; and the child was suffering so much from dyspnœa, that the slightest alteration from the upright position almost produced suffocation. The pulse was very rapid and indistinct; and it seemed that a fatal issue was imminent, the more so because the case had been treated with the greatest care and attention, and the only remedy I had confidence in under such circumstances had been already tried. On enquiring, however, as to the dose, I found that a much smaller one than I had often prescribed had been ordered, and I strongly urged another trial of the liq. potas iod. c. in drachm doses every four hours. There seemed but little chance; but happily on the following day the breathing was less laboured, and the child was actually able to lie down with the head but slightly raised. Suffice it to say that a continuance of the treatment produced the entire removal of the dropsy; and when I called again in about a week, the little girl was playing about almost well. She had no return of the disease.

In conclusion, I wish to make a few remarks upon a subject confessedly of the utmost importance, and which has been frequently discussed of late, namely, the means of preventing the spread of scarlet fever. It is without doubt a very great scourge in this country, and no very satisfactory conclusion appears to have been arrived at as to how it is to be prevented, or how its progress may be stayed. Opinions vary much upon the subject; but, after a long professional life, I hope I may be excused if I express, without any desire to dogmatise, the conclusions which my own experience has led me to adopt. Without discussing how scarlet fever first originates in any particular district, I dwell upon the mode of its propagation, and how it gradually increases in severity. A boy at school gets an attack of mild scarlet fever, and is sent home to a distance; his father's house is recently built and well situated, and the locality is healthy. This boy passes through a mild regular attack, and recovers. But soon a younger child is attacked, and then the nurse; and both rapidly die. Another child dies; and in a day or two the mother also; then a fine boy of four years old is seized in the morning, and dies at night in the stage of collapse, without presenting any of the distinctive marks of scarlet fever. One child, the eldest, only remains, and he is sent to an old almost unoccupied farmhouse, goes through a mild attack of scarlet fever, and recovers. The other members of the household are sent away to the seaside, and an examination of the premises

is made to ascertain, if possible, the cause of this dreadful mortality, all the deaths having occurred in the course of one week. I should say there was no scarlet fever in the neighbourhood at the time of this sad occurrence, nor did it spread to any other house in the village. I found nothing to complain of in the house—a new parsonage, clean and convenient in all its arrangements; but there were two water-closets; one upstairs for the ladies, and the other on the ground floor for general purposes. The waste-pipe of the former emptied itself into a covered drain running along the bottom of a fence to a great distance from the house; the latter discharged its contents into a square open bin situated close to the back door. Into this latter closet all the excreta of the patients and bedroom slops had been emptied; and to the exposure of them to the air in the open bin I attribute the increased virulence and fatality of the disease after its first importation into the house.

Very recently I was summoned to a neighbouring town to visit a young gentleman at school there; but being out of the way at the time, another practitioner from Norwich went in my stead. This was on a Sunday. On the previous Friday the boy had been playing cricket, and was not ill till that night. My friend found him moribund, and he died in a few hours. He seemed to have died of some acute attack in the head, and it was difficult to say what killed him in so short a time.

After a few days I was called in consultation to another pupil of the same school, but who had been sent to his own home, as his father resided in the same town. He was fourteen years of age, and previously in good health; and I found him labouring under a severe attack of scarlet fever, with eruption fully out all over him of a bright scarlet colour; with hot skin, rapid pulse, ichorous discharge from the nostrils, and drowsiness. His throat was sore, but not very much affected; he could be roused pretty easily; and I discovered no very dangerous complication. He was treated simply with liq. ammon. acet. and sp. æth. nit., the usual fever-mixture, and enemata when required. The nostrils were treated with nitrate-of-silver solution applied on a brush. I saw him again next morning, and found him not at all worse, and the nostrils decidedly better. Before I left the house I particularly cautioned his father not to allow any of the excreta to be put down a water-closet, but to be at once removed and emptied into holes in the garden, at a distance from the house, and covered with mould. I heard afterwards that he made a good recovery. But three other cases occurred in boys from the same school, two of whom died, showing a mortality of three out of five cases. My patient's brother had afterwards a very slight attack; his father had a little sore-throat; but his mother, a delicate lady,

who nursed him night and day throughout his illness, escaped the contagion altogether.

The result of my experience and my firm belief is, that scarlet fever, whatever may be the nature of its infectious principle, is propagated and intensified by the exposure of the excreta of those who are affected by it, and that they are the pabulum on which its miasm, whatever it may be, delights to feed. I am equally convinced that a speedy removal and disinfecting of these excreta will prove the best means of rendering the disease milder in its character, and less likely to spread its venomous influence in the localities in which it may chance to prevail.—*St. George's Hospital Reports*, 1870, *vol.* v. *p.* 55.

3.—ON THE ETIOLOGY OF TYPHOID FEVER.

By Dr. John Ewens, Cerne Abbas, Dorset.

[Dr. Ewens has long had the opinion that typhoid fever is not in the ordinary sense of the word contagious. In country places, a more correct conclusion on such matters can generally be arrived at than in large towns, where the origin of cases cannot be so easily traced].

In the spring of 1854 or 1855 (a very dry season) an epidemic of typhoid of a very severe character occurred in the hamlet of Whatcombe, about half a mile from the village of Whitchurch, in this county. It was entirely confined to the inhabitants of the houses which obtained their drinking water from a well (situated immediately opposite the doors of three cottages), the mouth of which was much below the level of the surrounding ground, and consequently easily admitted of the flow of refuse matters, thrown out of the houses, into the well. Adjoining houses, which obtained water from other sources, were free from the disease; and the neighbouring village of Whitchurch, with which there was constant communication, was absolutely free from typhoid, with the exception of two men, who worked at Whatcombe, and were accustomed to take their midday meal at a cottage near the well, and, doubtless, drank of the water. Suspicion soon fell upon the well, which, being examined, was found in a very impure state. It was closed *pro tem.*, and no other case occurred. In due course it was cleansed, and the surrounding ground raised, with good results.

In another village an epidemic of typhoid broke out in two adjoining cottages, every inmate of which (eight in all) was attacked, and three died. The cause was easily traced to impure water. Two of the nurses were attacked, were removed,

did not communicate the disease, and made good recoveries.

The village of Cheselbourne, near here, was formerly often the scene of more or less severe epidemics of typhoid fever. The inhabitants (about 440 in number), almost without exception, obtained their water from a stream which was exposed to contamination from various sources. In dry seasons, when the water is low, the impurity is less, owing to the absence of surface water, which, of course, brings foul matters from the farm-yards, which otherwise would drain away into the soil, and it so happened that typhoid was always most prevalent in wet seasons. After a very severe epidemic, urgent representations being made in the proper quarter, some wells were sunk, and since that time there has been a marked difference in the health of the place as regards typhoid fever. Sporadic cases have now and then occurred, as, in spite of all that can be said, people are always to be found who prefer drinking foul water, easily obtained, to going a short distance to get pure water.

One more case occurs to me. The inhabitants of two cottages, isolated, who ordinarily obtained their water from a well, were driven, through the well being out of repair, to get water from a small stream flowing down from a village where typhoid was epidemic, and which must have received poisonous dejecta. Some of these people were speedily attacked with the disease.

My experience leads me to confirm Dr. Latham's opinion as to the *comparative* rarity of nurses., &c., attending on the sick becoming affected. That such cases do happen cannot be denied, and I have sometimes been at a loss to explain them consistently with the idea of the ordinary non-contagious theory. Dr. L.'s explanation removes in a measure the difficulty. As a rule, I find it much more common to meet with only one or two cases in a family, and it is the exception for more to be affected at once. It is very desirable, if possible, that the question should be settled, as medical practitioners are constantly interrogated as to the danger of infection, and it is not always easy to explain the peculiar circumstances of the case so as to satisfy the minds of timid people. I have heard of medical men speaking of the disease as the "least contagious kind of fever," I suppose because they find it rarely spread by what appears to them to be contagion. But might we not on the same principle (if the reasoning were correct) say that a case of scarlatina, which, as we all know, may prove a solitary case, was less contagious than another which proved the nucleus of a severe and wide-spread epidemic? But the solitary case is the exception generally in scarlatina, and due to some cause not always easy to determine. I believe it will be found, as a rule, that wells sunk through a sandy soil are more likely to be contaminated by the proximity of a cesspool than

those sunk in chalk, and this perhaps may be the reason why manure heaps are often seen very near the mouth of a well in the latter stratum without the water getting impure.—*Lancet, August* 12, 1871, *p*. 250.

4.—ON THE ETIOLOGY OF CONTINUED FEVER.

By Dr. P. W. LATHAM, Fellow of Downing College, Cambridge, and Physician to Addenbrooke's Hospital.

[In the *Lancet* of July 1st, the following account, on the authority of Dr. Flint, is given of the propagation of typhoid fever; and an annotation is appended that "it would scarcely be possible to devise an experiment which would better test the communicability of typhoid fever by contagion than this":—]

"A stranger, who had been sick for several days, stopped from off a stage coach at a tavern in a little village eighteen miles from the city of Buffalo, called North Boston, and, after a few days, died. The village consisted of nine families, closely grouped together around the tavern. All these families, save one named Stearns, used water from a common well, and visited the sick stranger at the tavern. Prior to the arrival of this stranger there had been no typhoid fever there, but twenty-three days after his arrival a member of the landlord's family was taken down with the disease. Other cases quickly followed, and in a month more than one half of the population, numbering forty-three, had been affected, and ten had died. Stearns' family, being on ill terms with the tavern keeper and most of the other families, who were his tenants, did not visit the sick stranger, and had been forbidden the use of the common well, and as they alone, of all the families immediately surrounding the tavern, escaped the disease, he was accused of poisoning the water of the said well—a charge which chemical analysis showed to be entirely unfounded."

Most certainly these facts conclusively prove the *communicability* of typhoid fever, but I hesitate to accept them, though with considerable diffidence, as any proof that the disease is *directly contagious*. An equally remarkable instance, which I shall presently relate, of the spread of the disease, came under my own observation three months ago, and seemed decidedly to prove that the disease had spread by personal contact; further investigation showed the contrary.

The theory which my own experience leads me to support is as follows:—1. That *almost* invariably the disease proceeds from a special poison contained in the alvine excreta. 2. That this poison is *directly* introduced into the alimentary canal,

either in the food or, most generally, in the water drunk. 3. That it is not yet proved that the poison is contained in, or is disseminated by, the exhalations from drains, privies, &c., or that it can be absorbed into the system through breathing air contaminated with such exhalations.

The epidemic at North Boston lends support to my first point, and Dr. Flint himself says:—"The fact that all the families in which the disease prevailed were supplied with water from a common well, and the fact that the family in which no case occurred did not obtain water from this well, afford ground for supposing that a virus derived from the excreta was conveyed in the water drunk." Though the stranger died within a few days after coming to the tavern, it was not until twenty-three days after his arrival that the first case occurred. This is a long period of incubation, assuming that the disease was contagious, but not at all too long if we assume that the cesspool or drain into which the excreta was poured was leaky, and so allowed the poisonous material gradually to filter through the ground into the well.

The epidemic to which I have referred above as coming under my own observation occurred at Harston, a small village about five miles from Cambridge. Between the 1st of January and the end of April of the present year, no less than twenty patients were placed under my care in Addenbrooke's Hospital suffering from typhoid fever. A large proportion, and those the most severe cases, were sent from Harston, where I learnt that whole families were being affected, and several deaths had occurred. I wrote to my friend Mr. Trestrail, asking him to investigate what I had heard respecting the introduction of the fever into the village, also for information about the wells and drainage, stating my belief that the disease was spreading through contaminated drinking water. He replied:—"I find that you are correct in stating that there is a distinct history of the introduction of typhoid fever into this village. It appears that a man who had barely recovered from the fever came as a lodger to Mrs. P., who resides in the village, and whose child was subsequently attacked. The second case was that of a Mrs. C., a patient of mine, who had been in the house of Mrs. P. occasionally. Mrs. C.'s sister was afterwards taken ill with typhoid fever, The next case was that of a child who had also been to see the child P. frequently. The next two cases were persons living very near; and then cases aronnd them. Nearly all the cases have been near each other, where the houses are overcrowded—no gardens nor any proper drainage. It is usual for these persons to drink water from one of the *springs*. Now these springs are continually running at the rate of many gallons per minute; they come from

upwards of 200 feet below the surface, and the water is conducted up from this distance by tubes. It is therefore highly improbable that these springs can become contaminated."

Here, then, we have the disease introduced into a district and spreading rapidly, the first persons attacked being in direct communication with each other, and the drinking water not liable to contamination. Evidently a clear proof of its contagiousness! But wait. In a subsequent letter Mr. Trestrail writes: "I have discovered the cause of the spread of the fever—namely, there is running directly through the village a stream of water, into which nearly all the drains from the houses enter. The water of this stream has been generally used by the poor for cooking purposes, as the springs are at some little distance from their houses. Since the attention of the people has been called to the great importance of using only spring water I have not heard of any fresh case of fever in the village."

The outbreak of typhoid fever last year in Islington is strongly corroborative of the view that the disease spreads in the manner I have mentioned. As stated in the *Lancet* for Nov. 26th, 1870, at a certain dairy the milk cans were washed in water taken from a tank communicating with the drains, and a little water of course remained behind: there was no charge of gross admixture of water with the milk. The dairyman himself died of typhoid, and, of 140 families supplied with the milk, 70 contracted the disease. The majority of the cases occurred in close proximity to the dairy; but "in one long road a mile and a half from the dairy there were three families thus supplied with milk; two of these had typhoid in them, and they were the only houses in which the malady occurred."

Another point against the contagiousness of the disease is the rarity of its infecting, in hospitals, the physicians, nurses, or the other patients. I have, however, seen this, happen, and I have no doubt that such cases have led to the assumption that the poison is oftener introduced by way of the trachea than by way of the œsophagus; but, to my mind, the simple explanation is, that after examining the rose spots on the back or abdomen, or after auscultating the chest, the hands of the physician have been imperceptibly soiled by contact with the skin or linen; as he continues his round he perhaps unthinkingly brushes his hand across his lips, and so the poison finds an entrance. With respect to the nurses, there is sometimes, after removing the soiled linen of the patients or the bed-pans, not that scrupulous attention to cleanliness which is so essential, and thus, eating or serving out the diets of other patients with unwashen hands, some little speck of fever-poison is introduced with the food. I say "in hospitals;" for in private houses the nurses and friends are often exposed to the same

influence which caused the illness of the patient, and so may be infected, but not necessarily through the patient.

I would, in conclusion, make two practical suggestions with regard to the treatment of typhoid-fever patients:—

1. That every evacuation, as soon as passed, should be disinfected with carbolic acid.

2. That no surface-well water or pump water, whether boiled or not, should be used in a district where there are any cases of typhoid fever, but that all drinking water should be obtained from some spring not liable to contamination.

I need not say that this precaution is of the greatest importance to the patients themselves. I am quite sure that neglect of it gives rise to many of the relapses in typhoid fever cases, perhaps more frequently than any other cause except the too early administration of solid food. And with respect to this last point, though I am now going beyond the subject of my paper, I would just point out the supreme importance of the thermometer in telling us when solid food may safely be given. It should not be given until the temperature of the patient at 8 a.m. and 6 p.m. has remained, for two days at least, about the normal point, or between 98° and 99° F. The patient's tongue may be clean and moist, the appetite ravenous, the patient crying out for food, and yet the typhoid ulcers still unhealed. The thermometer alone will tell us this; it will probably show at this stage an evening temperature of about 101° F., with a morning temperature 1·6° to 2° lower; and a mutton-chop now might be sufficient to induce fresh irritation of the intestinal ulcers, fatal hemorrhage or perforation. It is only after the evening temperature has remained, on at least two successive days, below 99° F. that we can be sure the ulcers have healed, and that solid food may be given without risk.

Out of twenty patients mentioned as admitted into Addenbrooke's Hospital only one died; but had not careful thermometric observations been made, the mortality would, I think, have been greater. In two who recovered there was, after the fortieth day of the disease, considerable hemorrhage from the bowels, and this after the tongue had become moist and clean, the motions perfectly formed, and the patients for some days have been bitterly complaining at only being allowed beef-tea, milk, and wine. The evening temperature still rose as high as 101° F., and no solid food was given. If it had been, I have little doubt it would have turned the scale in each case against the patient's recovery.—*Lancet, July* 15, 1871, *p.* 81.

5.—ON THE MODE OF EMPLOYMENT OF SULPHATE OF QUININE IN SIMPLE (NON-PERNICIOUS) INTERMITTENT FEVER.

By Dr. Auguste Nonat, formerly Physician to the Hôpital de la Charité, and Vice-Professor at the School of Medicine, Paris.

[Sulphate of quinine is used too indiscriminately in the treatment of intermittent fever, without a clear idea as to the dose suitable to the case. Either large doses are exhibited where smaller ones would have sufficed, or small doses have been vainly employed and the use of the drug thrown up where large doses were necessary, and would have afforded satisfactory results. The following rules of practice are the result of much careful study of the subject, and by attending to them quinine may be exhibited with a certainty of success, and a cure often obtained in cases which had been given up in despair by others.]

The mode of employment of sulphate of quinine in simple intermittent fever must depend upon four main conditions.

1. We must take into account the *intensity* of cause, and therefore make inquiry concerning the locality where the disease was contracted; for instance, Algeria, Paris, Rome, Sologne; the cause being more or less powerful according to the infecting localities.

2. The oldness of the complaint must be taken into serious consideration. The older the fever, the stronger must be the dose of quinine.

3. The type of the fever. Thus quartan fever requires a much larger dose of quinine than the quotidian or tertian type.

4. The degree of tumefaction of the spleen And, indeed, when the spleen is not enlarged, *cæteris paribus*, the dose of sulphate of quinine should be smaller; and on the other hand, the larger the tumefaction of the spleen, the more must we increase the dose of the remedy.

In quartan fever the patient has two good days out of three; he may do his work, attend to his ordinary occupations, and not call in the doctor. Meanwhile the complaint becomes more intense—*vires acquirit eundo*. This, therefore, turns out an old case, and may be coupled with my second condition; namely, the *oldness* of the disease. Indeed, it is much on this account that the quartan type is less amenable to treatment than the tertian or quotidian.

We may therefore reduce to three the rules to which I have referred, and put it down that, in order to determine the due amount of remedy which it is necessary to exhibit, we must take into account *the intensity of the cause, the oldness of the disease, and the state of tumefaction of the spleen.*

Next comes the question of the amount of quinine which it is suitable to administer at the outset of the treatment. When called to the bedside of a patient laying under fever, by what dose must we begin? In a general way the dose will vary from 5 to 23 grains. The *minimum* dose will be administered in *recent* cases of simple intermittent fever unattended by tumefaction of the spleen; the *maximum* in *old* cases of intermittent, accompanied by considerable enlargement of spleen, whilst the intermediary doses will suit intermediary cases. But it should be borne in mind that *whenever* the spleen is enlarged, at least 10 to 12½ grains should be administered every day at the outset.

This remark leads to another important question; namely, the order of administration of doses, with regard to strength, and the duration of the use of quinine.

Experience leads me to set down that the maximum dose must be immediately given to commence with, that it must be continued for four or five days, and that we must then go on gradually decreasing the dose, till complete resolution of the enlargement of spleen has taken place. Thus, for example, if we begin with 23 grains, I should say 23 grains for five or six days, then 15 grains for five or six days more, then 7½ grains for the same space of time, and so on. The total duration of exhibition of the drug will generally take about three weeks; still after this time however we must not suspend its use, but continue giving some small doses from time to time, in order to keep up the action of the medicament.

Another important point is the necessity of keeping up the patient's strength during treatment, and of giving him a proper amount of food. I have observed that sulphate of quinine is far better tolerated by patients who are properly fed. Thus, on the very first day, as soon as the access is stopped by quinine, I am accustomed to allow my patient his ordinary number of meals, or only to lessen them according to circumstances. The stomach puts up better with the remedy, and the gastralgic effects of quinine are thus much seldomer noticed than when diet is enforced.

The mode of the treatment which I have now described succeeds equally well with the quartan type as with the others, which shows that the quartan type proves refractory only because the treatment is not appropriate and consists of too feeble doses at the outset.

Practically, and to sum up; to cure intermittent fever we must exhibit sulphate of quinine in due amount and in proper order, to counteract the effects of the paludal poison, when there exists no complication, no inflammation, on the part of the spleen—but when this complication does exist we must master it by local applications.

A word in respect to arsenic. It may be broadly stated that arsenic is not equal to the sulphate in ordinary cases of intermittent. I have found it act on the cachectic state of the patient, but not on the local lesion, and therefore it leaves the patient exposed to a recurrence of the fever.—*Practitioner, Oct.*, 1871, *p.* 193.

6.—DUST AND DISEASE.

By Prof. JOHN TINDALL, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution of Great Britain.

After some preliminary experiments and observations on the polarisation of light by fine dust, by the sky, and the coarser particles of smoke, Professor Tyndall proceeded :—

In looking at this illuminated dust, we may ask ourselves what it is. How does it act, not upon a beam of light, but upon our own lungs and stomach? The question at once assumes a practical character. We find, on examination, that this dust is of organic matter—in part living, in part dead. There are among it particles of ground straw, torn rags, smoke, the pollen of flowers, the spores of fungi, and the germs of other things. But what have they to do with the animal economy? Let me give you an illustration to which my attention has been lately drawn by Mr. George Henry Lewes, who writes to me thus.

"I wish to direct your attention to the experiments of Von Recklinghausen, should you happen not to know them. They are striking confirmations of what you say of dust and disease. Last spring, when I was at his laboratory in Wurzburg, I examined with him blood that had been three weeks, a month, and five weeks, out of the body, preserved in little porcelain cups under glass-shades. This blood was living and growing, Not only were the amœba-like movements of the white corpuscles present, but there were abundant evidences of the growth and development of the corpuscles. I also saw a frog's heart still pulsating which had been removed from the body (I forget how many days, but certainly more than a week). There were other examples of the same persistent vitality or absence of putrefaction, Von Recklinghausen did not attribute this to the absence of germs—germs were not mentioned by him; but when I asked him how he represented the thing to himself, he said the whole mystery of his operation consisted in keeping the blood *free from dirt*. The instruments employed were raised to a red heat just before use, the thread was silver-thread and was similarly treated, and the porcelain

cups, though not kept free from air, were kept free from currents. He said that he often had failures, and these he attributed to particles of dust having escaped his precautions."

Professor Lister, who has founded upon the removal or destruction of this "dirt" great and numerous improvements in surgery, tells us of the effect of its introduction into the blood of wounds. He informs us what would happen with the extracted blood should the dust get at it. The blood would putrefy and become fœtid; and when you examine more closely what putrefaction means, you find the putrefying substance swarming with organic life, the germs of which have been derived from the air.

Another note which I received a day or two ago has a bearing particularly significant at the present time upon this question of dust and dirt, and the wisdom of avoiding them. The note is from Mr. Ellis, of Sloane Street, to whom I owe a debt of gratitude for advice given to me when sorely wounded in the Alps. "I do not know," writes Mr. Ellis, "whether you happened to see the letters, of which I enclose you a reprint, when they appeared in the *Times*. But I want to tell you this in reference to my method of vaccination as here described, because it has, as I think, a relation to the subject of the intake of organic particles from without into the body. Vaccination in the common way is done by scraping off the epidermis, and thrusting into the punctures made by the lancet the vaccine virus. By the method I use (and have used for more than twenty years) the epidermis is lifted by the effusion of serum from below, a result of the irritant cantharadine applied to the skin. The little bleb thus formed is pricked, a drop of fluid let out, and then a fine vaccine point is put into this spot, and after a minute of delay it is withdrawn. The epidermis falls back on the skin and quite excludes the air—and not the air only, but what the air contains.

"Now mark the result: out of hundreds of cases of revaccination which I have performed, I have never had a single case of blood-poisoning or of abscess. By the ordinary way the occurrence of secondary abscess is by no means uncommon, and that of pyæmia is occasionally observed. I attribute the comparative safety of my method, first, to the exclusion of the air and what it contains; and, secondly, to the greater size of the apertures for the inlet of mischief made by the lancet."

I bring these facts forward that they may be sifted and challenged if they be not correct. If they be correct, it is needless to dwell upon their importance; nor is it necessary to say that, if Mr. Ellis resigned himself wholly to the guidance

of the germ-theory, he could not have acted more in accordance with the requirements of that theory than he has actually done. It is what the air contains that does the mischief in vaccination. Mr. Ellis's results fall in with the general theory of putrefaction propounded by Schwann, and developed in this country with such striking success by Professor Lister. They point, if true, to a cause distinct from bad lymph for the failures and occasional mischief incidental to vaccination; and, if followed up, they may be the means of leaving the irrational opposition to vaccination no ground to stand upon, by removing even the isolated cases of injury on which the opponents of the practice rely.

We are now assuredly in the midst of practical matters. With your permission, I will recur once more to a question which has recently occupied a good deal of public attention. You know that as regards the lowest forms of life, the world is divided, and has for a long time been divided, into two parties, the one affirming that you have only to submit absolutely dead matter to certain physical conditions to evolve from it living things; the others, without wishing to set bounds to the power of matter, affirming that in our day no life has ever been found to arise independently of pre-existing life. Many of you are aware that I belong to the party which claims life as a derivative of life. The question has two factors: the evidence, and the mind that judges of the evidence; and you will not forget that it may be purely a mental state or bias on my part that causes me throughout this discussion, from beginning to end, to see on the one side dubious facts and defective logic; and, on the other side, firm reasoning and a knowledge of what rigid experimental inquiry demands. But, judged of practically, what, again, has the question of spontaneous generation to do with us? Let us see. There are numerous diseases of men and animals that are demonstrably the products of parasitic life, and such diseases may take the most terrible epidemic forms, as in the case of silkworms of France in our day. Now, it is in the highest degree important to know whether the parasites in question are spontaneously developed, or are wafted from without to those afflicted with the disease. The means of prevention, if not of cure, would be widely different in two cases.

But this is by no means all. Besides these universally admitted cases, there is the broad theory now broached and daily growing in strength and clearness—daily, indeed, gaining more and more of assent from the most successful workers and profound thinkers of the medical profession itself—the theory, namely, that contagious disease generally is of this parasitic character. If I had heard or read anything since to cause me

to regret having introduced this theory to your notice more than a year ago, I should here frankly express that regret. I would renounce in your presence whatever leaning towards the germ-theory my words might then have betrayed. Let me state in two sentences the grounds on which the supporters of the theory rely. From their respective viruses you may plant typhoid fever, scarlatina, or small-pox. What is the crop that arises from this husbandry? As surely as a thistle rises from a thistle-seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox virus into small-pox. What is the conclusion that suggests itself here? It is this:—That the thing which we vaguely call a virus is to all intents and purposes a *seed;* that in the whole range of chemical science you cannot point to an action which illustrates this perfect parallelism with the phenomena of life—this demonstrated power of self-multiplication and reproduction. There is, therefore, no hypothesis to account for the phenomena but that which refers them to parasitic life.

And here you see the bearing of the doctrine of spontaneous generation upon the question. For if the doctrine continue to be discredited as it has hitherto been, it will follow that the epidemics which spread havoc amongst us from time to time are not spontaneously generated, but that they arise from an ancestral stock whose habitat is the human body itself. It is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon disease-germs, which no bad air or foul drains can create, but which may be pushed by foul air into virulent energy of reproduction. You may think that I am treading on dangerous ground, that I am putting forth views that may interfere with salutary practice. No such thing. If you wish to learn the impotence of medical science and practice in dealing with contagious diseases, you have only to refer to a recent Harveian oration by Dr. Gull. Such diseases defy the physician. They must burn themselves out. And, indeed, this, though I do not specially insist upon it, would favour the idea of their vital origin. For if the seeds of contagious disease be themselves living things, it will be difficult to destroy either them or their progeny without involving their living habitat in the same destruction.

And I would ask you to be cautious in accepting the statement which has been often made, and which is sure to be repeated, that I am quitting my own *métier* when I speak of these things. I am not dealing with professional questions. I am writing no prescription, nor should I venture to draw any

conclusion from the condition of your pulse and tongue. I am dealing with a question on which minds accustomed to weigh the value of experimental evidence are alone competent to decide, and regarding which, in its present condition, minds so trained are as capable of forming an opinion as on the phenomena of magnetism and radiant heat. I cannot better conclude this portion of my story than by reading to you an extract from a letter addressed to me some time ago by Dr. William Budd, of Clifton, to whose insight and energy the town of Bristol owes much in the way of sanitary improvement.

"As to the germ-theory itself," writes Dr. Budd, "that is a matter on which I have long since made up my mind. From the day when I first began to think of these subjects, I have never had a doubt that the specific cause of contagious fevers must be living organisms. It is impossible, in fact, to make any statement bearing upon the essence or distinctive characters of these fevers, without using terms which are of all others *the most distinctive of life.* Take up the writings of the most violent opponent of the germ-theory, and, ten to one, you will find them full of such terms as 'propagation,' 'self-propagation,' 'reproduction,' 'self-multiplication,' and so on. Try as he may—if he have anything to say of those diseases which is characteristic of them—he cannot evade the use of these terms or the exact equivalents to them. While perfectly applicable to living things, these terms express qualities which are not only inapplicable to common chemical agents, but, as far as I can see, actually inconceivable of them."

Once, then, established within the body, this evil form of life, if you will allow me to call it so, must run its course. Medicine as yet is powerless to arrest its progress, and the great point to be aimed at is to prevent its access to the body. It was with this thought in my mind that I ventured to recommend, more that a year ago, the use of cotton-wool respirators in infectious places. I would here repeat my belief in their efficacy if properly constructed. But I do not wish to prejudice the use of these respirators in the minds of its opponents by connecting them indissolubly with the germ-theory. There are too many trades in England where life is shortened and rendered miserable by the introduction into the lungs of matters which might be kept out of them. Dr. Greenhow has shown the stony grit deposited in the lungs of stone-cutters. The black lung of colliers is another case in point. In fact, a hundred obvious cases might be cited, and others that are not obvious might be added to them. We should not, for example, think that printing implied labours where the use of cotton-

wool respirators might come into play, but I am told that the dust arising from the sorting of the type is very destructive of health. I went some time ago into a manufactory in one of our large towns, where iron vessels are enamelled by coating them with a mineral powder and subjecting them to a heat sufficient to fuse the powder. The organisation of the establishment was excellent, and one thing only was needed to make it faultless. In a large room a number of women were engaged covering the vessels. The air was laden with the fine dust, and their faces appeared as white and bloodless as the powder with which they worked. By the use of cotton-wool respirators, these women might be caused to breathe air more free from suspended matters than that of the open street. Over a year ago I was written to by a Lancashire seedsman, who stated that during the seed-season of each year his men suffered horribly from irritation and fever, so that many of them left his service. He asked me could I help him, and I gave him my advice. At the conclusion of the season this year he wrote to me that he had simply folded a little cotton-wool in muslin, and tied it in front of the mouth; and that he had passed through the season in comfort, and without a single complaint from one of his men.

The substance has also been turned to other uses. An invalid tells me that at night he places a little of the wool before his mouth, slightly moistening it to make it adhere; that he has thereby prolonged his sleep, abated the irritation of his throat, and greatly mitigated a hacking cough from which he had long suffered. In fact, there is no doubt that this substance is capable of manifold useful applications. An objection was urged against the use of it: that it became wet and heated by the breath. While I was casting about for a remedy for this, a friend forwarded to me from Newcastle a form of respirator invented by Mr. Carrick, an hotel-keeper at Glasgow, which meets the case effectually, and, by a slight modifition, may be caused to meet it perfectly. It consists of a space under a partition of wire-gauze, intended by Mr. Carrick for "medicated substances," and which may be filled with cotton-wool. The mouth is placed against an aperture, which fits closely round the lips; and the air enters the mouth through the cotton-wool by a light valve, which is lifted by the act of inhalation. During exhalation this valve closes; another breath escapes by a second valve into the open air. The wool is thus kept dry and cool; the air passing through it being filtered of everthing it holds in suspension.*—*British Medical Journal, June* 24, 1871, *p.* 661.

* Mr. Ladd, of Beak Street, sells these respirators.

7.—ON CANCER.

By Campbell De Morgan, Esq., F.R.S., Surgeon to the Middlesex Hospital.

What is the origin of Cancer? Does the disease consist solely in a local change of structure; or is it the local determination of a general morbid condition of the blood; or are the blood and the tissue equally interested in its formation?

It is not surprising that surgeons have come to regard cancer as from the first a constitutional or blood disease. Indeed, it is no easy matter to account for all that we observe in cancer without adopting some such view. But, on the other hand, there are many facts which oppose themselves to it, and there are other modes of accounting for some of those characters which most strongly support it. I do not think that the importance of the question is in general so fully realised as it should be. Some, indeed, seem to consider that, practically, the question is not worth discussing. Yet, surely, if cancer be a constitutional disease, we must strive to find some corrective to the constitutional taint. And we should try in that case to determine whether the local disease is the result of some error in the organising or nutrient function of the tissues, or whether it is the actual deposit of some poison previously existing in the blood. If the former, our aim must be to find out the source of this error; if the latter, to learn the nature of the poison, and to be able to detect it. Then we might have a reasonable hope of being able to do, what has as yet proved beyond our reach—viz., to cure or even to modify cancer by general treatment. But if cancer be, at any period of its existence, a purely local disease, we should hope that by earlier and more complete removal better and more generally permanent results may be obtainable than we can at present boast of.

Admitting fully the difficulties which lie in the way of arriving at a perfectly satisfactory conclusion, and notwithstanding the high authority of those who adhere to the more commonly received opinion, I may state that the more I consider the question, the more do I hold to the view which has been so ably advocated by my late colleague, Mr. Moore, and which is maintained by many in this country, and by Virchow and many German pathologists. That there may be a constitutional tendency to cancer I fully admit, but that it is in any sense to be regarded as a blood disease seems to me to require stronger evidence than we have yet obtained. I should say that cancer and allied diseases partake more of the nature of parasitic disease than of the results of previous blood-poison. The final destiny of such tumour will depend, not on any special chemical or nutrient element, but on the nature of the growth itself.

This seems the view taken by Mr. Huxley. In his presidential address to the Association for the Advancement of Science (1870), he says, speaking of xenogenesis, "It is only in pathology that we find any approximation to true xenogenesis; and it is furnished by the various structures in which, under the influence of certain external conditions, elements of the body which should have developed in due subordination to its general plan, set up for themselves, and apply the nourishment they receive to their own purposes. From such innocent productions as corns and warts there are all gradations to more serious tumours, and in the terrible structures known as cancers, the new growth has acquired powers of reproduction and multiplication, and is only distinguished by form from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism."

To a certain extent--to a great extent indeed—I admit a constitutional tendency to this disease; but I admit equally a constitutional tendency to warts, or to fatty and other simple tumours. I cannot suppose that there is any material in the body which must work itself out of it, and which selects this situation or that indiscriminately, or perhaps makes use of one in preference to another from finding a tissue better prepared for its reception. I cannot suppose that there are various kinds of this morbid material—one breaking out as scirrhus, another as epithelioma, &c. If the cancer poison be of one kind only, we should surely find it more frequently appearing in the same subject at one time as encephaloid, at another as scirrhus, at another as epithelioma, or some other form of malignant disease. Yet this is seldom the case, though it does occur. I quite admit that the nature of a cancer will be modified by the character of tissue in which it appears, and by the general activity of the tissues. We may find this in gout, or rheumatism, or syphilis. But the sufferer from either of these diseases will be liable to attacks of it at different times in every possible form, or in many forms at once, and this we do not so often find in cancer. Other general or blood disease, as the exanthemata, will produce structural change only in certain tissues. Now, however mild and however limited the local manifestations of any recognised blood disease may be, we find evidence of a general affection of the system as well. We cannot say as much for cancer.

We may consider in detail those special characters which have led to so firm a belief in the blood origin of cancer. Perhaps that which seems to be the most conclusive evidence to the large majority of those who hold this view is the almost constant recurrence of the disease after removal. Entire and permanent immunity does occur, but it is undoubtedly rare. In

such cases it would be said, first, that the blood-poison having worn itself out, or been eliminated by the formation of the tumour, no further development took place when the local tumour was removed; or, second, that two conditions were necessary for the formation of cancer—one, the blood-poison; the other, a fit state of tissue in which it might manifest itself; and that, a tumour having been removed, there remained no fit nidus for its reproduction. The first view is that taken by Mr. Simon, who considers that the tumour stands in the same relation to the *materies morbi* as a secreting gland does to the matter eliminated by it. The second view is that maintained by Mr. Paget. It need scarcely be remarked that these two conceptions necessarily lead to opposite modes of practice; for if the tumour be an eliminative organ it would be undesirable to remove it at all. as the disease must find some outlet; and many surgeons, acting on this view, object to early operation. But if it may be that on the removal of a tumour no other tissue is in a condition to take on the diseased action, it would be well to operate early, if only on the chance of such a want of consent between the blood and the tissues.

These cases of permanent cure after operation are, we may admit, exceptional. The more common event is that, after removal of a tumour, in favourable cases, there is immunity for a longer or shorter period—some months or some years. Is the blood diseased during this time? Are the other tissues free from disease? It is most important here to consider the mode of recurrence of the disease. It is a recognised fact that cancer rarely returns either in an organ corresponding to its original seat, or indeed in any organ which is the usual seat of the primary disease. A cancer in the breast, *e.g.*, rarely returns in the opposite breast; so in the testicle, or the eye, or the side of the lip. Primary scirrhus, again is rare in the lymphatic glands, in the integuments of the trunk or limbs, in the lungs; while its by far most common seat is the female breast or the uterus. Now, supposing that recurrent cancer is a development, *de novo*, the result of a diseased state of blood in combination with a fitting tissue, should we not expect that very often the return would be in an organ which is the common seat of the disease? Take, for example, an excision of the female breast. The patient remains well for two years, and the disease then returns. If this is a new development dependent on constitutional causes, it would be strange indeed if we did not often find it taking place in the other breast, or in the uterus; still stranger that it should occur in nineteen cases out of twenty in tissues or organs which are rarely the seat of primary cancer. Yet such is the fact. Mr. Sibley, in his careful analysis of 520 cases of cancer observed in the Middlesex Hospital, did not find

one in which a recurrence of the disease took place in the ordinary seats of the disease. Surely this fact alone should make us pause ere we admit that a cancer-poison in the blood is one of the factors in the production of cancer. I quite admit the force of the argument that an operation may so change the tissue of the part operated on as to make it a fitting seat of future disease. But this will only apply to those cases in which the return is in or around the cicatrix, not to those where distant lymphatics or lungs or other internal organs are affected, the cicatrix remaining sound. And still the difficulty of accounting for the fact that, with a cancerous state of blood existing for a length of time, no recurrence takes place in the organs most prone to the disease would remain the same. The influence of local condition must be at least more powerful than that of blood.

And during all this time, as well as before the discovery of the original tumour, the patients are usually in robust health. Nothing can be more erroneous than the belief entertained by those who have not had much experience of the disease that there is a cachectic condition in the early stages of cancer. To use the words of Professor Humphry, "So much is said and written about cancerous cachexia, that cachexia comes to be regarded as a necessary associate of cancer. Hence physicians and surgeons rely upon it as a means of diagnosis, and conceive that a disease cannot be cancerous because the patient's health is good. Whereas, in reality, cancer, especially in early and middle life, fastens itself often, I would say oftenest, upon those who are well-nourished and florid, who seem the most healthy and robust, and so give promise of long life and vigour." This point is of the utmost practical importance; yet it is generally ignored, or rather the opposite opinion is generally entertained. Tumours are discovered, and are allowed to grow till too late for removal, the patient, or perhaps the medical attendant, considering that there was nothing of consequence in them because there was no appearance of cancerous cachexia. In reality, however, the fact that a tumour is developed in the breast of a person in otherwise sound health, and over thirty—a tumour painless, and only discovered when already of some size,—is enough to justify a more than suspicion that it is cancer. If cancer should ever be removed by operation, I suppose few will deny that the sooner it is done the better. But what chances of successful removal are daily thrown away owing to the presence of those very conditions which should most surely excite suspicion in the mind of the surgeon. I hope I may not be misunderstood in what has been here stated on this point. It refers to the large majority of cases. There is no doubt that cancer does attack those who have

been delicate, and have suffered from one complaint or another all their lives; but even in such persons there is usually nothing from which one could predicate the after development of cancer as one usually can in the states of health leading up to tuberculosis.

There is another circumstance in connexion with the recurrence of cancer after operation which to my mind is very significant. I have noticed, and it has been verified by the observation of many others, that concurrently with or following on, the development of cancer, small outgrowths of warty, or vascular, or dermoid structure are frequent. Now one would imagine that, if there were a cancer-poison in the blood these or one of them would become the seat of the disease. But it is never the case, although a large outbreak of cancer has taken place in other situations, such as are the usual seats after operations, but the rare ones independent of them. The frequent coexistence of cancer with other growths will be again alluded to.

The hypothesis of the double origin of cancer, a state of the blood and a state of local tissue fit for its invasion, presents another difficulty. It implies that this diseased blood-condition may exist, and be long present—indefinitely long for aught we know,—without any, even the slightest, deviation from robust health. Something allied to it may be seen in syphilis, but with a difference. A person may have a syphilitic taint, which will not for a length of time have any external manifestation. But should this person have an accident, or be in any way put out of condition, the influence of the poison will display itself. In cancer, on the contrary, whether previous to its recognised development or while it is present, or in the interval between operation and its recurrence, up to the time when cachexia has actually set in, any injury, a wound, a bruise, a fracture, may occur, and no influence of the disease will be manifested. Unless in the case of syphilis—and even there the presence of sound health rarely accompanies its presence in the system,—all blood diseases will be attended by a state of deviation from sound health, even when there is no external or local expression of disease.

There are other and stronger grounds perhaps for considering that the persistent redevelopment of cancer is no proof of the presence of a blood-poison. There are few forms of unencapsuled tumours which do not from time to time exhibit precisely the same tendencies to reproduction which we see in cancer itself. Fibroma, enchondroma, myxoma, sarcoma, myeloid, &c.—all may show the same tendency, though no trace of cancer structure can be detected. In many forms of tumour—as the recurrent fibroid, for example—the disease, after appa-

rently complete extirpation, returns only in the original neighbourhood. It may be remarked, too, that a long period may elapse between the operation and any appearance of return. But often we have a general dissemination as widespread as we ordinarily find in recurrent cancer. Thus, in a case of fibroplastic tumour which I brought before the Pathological Society, there were secondary tumours in the liver and kidney, and the lungs were remarkably studded with them. These secondary growths in the lung were all encapsuled, and in no place could any trace of cancer structure be found. In another case, recorded in volume xx. of the Pathological Society's Transactions, a widespread deposit of cartilaginous and of spindle-cell tumours in the abdomen and chest followed the removal of a cystic disease of the testicle two years previously. The original tumour contained cartilage-nodules, and was perhaps, as Virchow has pointed out, originally an enchondroma. No cancer structure could be found. Such cases are by no means rare: indeed, they are now so universally recognised that it is hardly necessary to do more than call attention to them.

Although, however, we have this widely recurrent dissemination in tumours not cancerous, there is often, it must be admitted, a difference in the seats of recurrence. In the fibrous or cartilaginous, or other non-cancerous growths, the secondary tumours appear in the line of the absorbents leading to the more central parts, as in the neighbouring lymphatic glands, or those still more removed, or else in the direct line of the venous circulation, as in the lungs or liver. But we rarely, if ever, find recurrent non-cancerous growths in organs out of the direct line—if we may so speak—of infection. Thus we should not expect to find a myeloid or a sarcomatous tumour of one limb followed by a similar outgrowth in the brain, or the muscles of a distant part, or in one of the other limbs. In cancer, scirrhus and encephaloid particularly, we are not surprised to find recurrence in any situation or in any tissue. This is, after all, a question of degree. Epithelioma is as truly cancer as scirrhus, yet we find very little disposition in it to contaminate distant organs. The neighbouring lymphatics and the parts surrounding them are the chief homes of secondary deposit and growth. It must be admitted, too, that cancer gives rise in its later stages to a cachexia far more marked, in proportion to the amount of actual structural disease, than is ever seen in the case of non-cancerous growths. But, as we shall see, the cancerous cachexia is the result for the most part of cancerous ulceration.

No doubt exists in the mind of any pathologist as to the mode of recurrence in many tumours, cancerous or non-cancerous. The diseased structure finds its way into the absorbents

or the veins. In the former it lodges and grows; in the latter it is carried away and deposited just as other minute emboli would be, and sooner or later grows at the seats of arrest. Mr. Paget relates a case in which the matter of an enchondroma was found in the lymphatics of the testicle, and had made its way by ulceration into the interior of the vena cava; the lungs were studded with secondary enchondromatous deposits; many small shrub-like growths were also attached to the inner membrane of the branches of the pulmonary artery. Nothing could be clearer than the course of the disease in this instance: a cartilaginous tumour in the testicle; cartilage-growth traced up the lymphatics of the cord; one of these growths ulcerating into the cava; particles of this growth washed away in the current of the blood; some adhering to the pulmonary artery, others, forced onwards to the smaller ramifications, arrested and forming encysted nodules of the disease. We have, in fact, the ordinary conditions of embolism; but the emboli are living and growing, instead of being mere inert exudations. It is far easier to explain the recurrence of disease, whether cancerous or not, in such cases, than in those which, without ever extending to distant organs, pertinaciously refuse to be eradicated from the site of their first development. Take keloid or some form of recurrent fibroid for example, which cannot surely be reckoned amongst blood diseases. In the former, especially, the whole course of the disease is so purely local that we could no more attribute it to a general cause than we could a wart or an atheroma or a nævus. The cicatrix keloid of Dieburg is frequently associated with the removal of some innocent tumour, as chronic mammary. As the wound heals, which it may do readily, the cicatrix assumes the peculiar character of keloid. The points where sutures have been inserted take on the same character. The affection increases up to a certain point, and there stops. Remove it, with a large amount of the healthy skin around it, and as healing takes place the keloid forms. Here, then, we have a condition dependent on the cicatrisation of an apparently healthy skin; stationary, or nearly so, when once formed, but recurring after removal, however freely the operation may have been performed. But while it will take place on cicatrices in some situations, wounds in other parts may heal without any appearance of the disease. Save in the disposition to recurrence in a cicatrix, there is no point of similarity to cancer or other malignant disease. The course of some recurrent fibroid tumours is very similar, but in them there is a far greater tendency to extension and to recurrence at a distance from the original seat of disease, yet still within narrow limits. Now, although these could under no circumstances be regarded as blood diseases, but must be the results of some

peculiar state of tissue, the tendency to recurrence is, I believe, far more difficult of explanation than it is in such diseases as recurrent enchondroma, where the morbid structure can be traced into absorbents and bloodvessels, or as cancer itself, where, besides in these, it can be found spreading in the connective-tissue spaces.

[Mr. De Morgan enters at some length upon the dissemination of cancer.]

We may start from the recognised physiological fact that every tissue has its own special power of reproduction. The blood brought to the tissues is the same; the tissue uses what it requires, and produces its like. I do not enter on the question of where, in the tissue, the power resides—whether in muscle alone or in cell. The experiments of Ollier have demonstrated that the scrapings of the soft inner layer of periosteum will form bone-nodules in whatever tissue they may be implanted. The recent practice of transplantation proves that epithelial cells planted on granulations will germinate and form layers of new epithelium. It is in tissues of low vitality and great tenacity of life that such experiments succeed; but they are sufficient to prove the fact. Hence we should expect that in cancer this growth from transplantation would be even more likely to occur, as it exceeds other tissues in the low vitality and tenacity of its elements. Intentional experiment has, of course, not been made on the human subject; but unintentional experiments have not been wanting. Several cases are recorded of men, whose wives have had uterine cancer, having themselves been victims of cancer of the penis. Of this I met with another instance only a short time ago. These *may* be mere accidental coincidents. They probably are, for there is no sufficient proof that cancer may be communicated from one person to another. Further evidence is still required on this point. There is no want of evidence of auto-inoculation. My colleague, Mr. Shaw, attended a patient in whom a pendulous breast, the seat at its lower part of ulcerated cancer, rubbed against the skin of the thorax. At the point of contact cancerous ulceration took place, the intervening skin between this and the fold of the mamma remaining healthy. Dr. Reineke has related two cases in which abdominal cancers were tapped, in error, with a trocar, and cancerous growth took place in the abdominal walls along the track of the puncture. Cancer is constantly found in the serous cavities at points opposed to those which have been the previous seat of the disease; and surgeons are familiar enough with the fact that, whenever a cancer is cut into in operation instead of cut out, the neighbouring parts become at once the seat of

widespread disease. Dr. Moxon showed at the Pathological Society a very interesting specimen, in which it was clear that numerous small nodules of epithelial cancer, occupying the lower lobes of the lungs, had been transplanted from a primary growth in the trachea. Dr. Dickinson lately brought a specimen to the same Society, showing numerous outgrowths in the peritoneum from the bursting of a spindle-celled sarcoma into the cavity. Nor does it, I think, admit of doubt, that in a case of my own, where encephaloid tumour within the cranium was followed by small cancerous growths at the lowest part of the cerebro-spinal sheath, the same kind of migration and implantation had taken place. The fact of the auto-inoculation of cancer and allied diseases is clearly established, and is now generally admitted.

What thus takes place on surfaces has been shown to occur within the tissues. That the absorbents and blood-vessels take up and disseminate cancer is admitted. There is as good evidence that the same may occur in any space through which minute particles can travel. The original observations of Van der Kolk have been confirmed, that dispersed cancer-cells may be found in the connective-tissue spaces of the gland to a considerable distance from the parent tumour. The same may be found in the connective tissue of fat. If careful sections be made around and beyond the cancer tumour, cells similar to those found in the tumour may be seen clustered here and there, and lying free, in the areolæ of the connective tissue. This occurs in parts which appear to be perfectly healthy, and it shows that we may, and probably do, often believe that in operation we have got beyond the range of disease when in reality these germs have spread beyond our reach. I may recall one case which illustrates this. In that of the patient just referred to as having had encephaloid at the lower part of the spinal cord, the original disease was an intraocular cancer. The eye had been extirpated, and it was believed that the operation had been effected beyond the range of the disease. But the cancer rapidly returned; and on carefully examining the stump of the optic nerve in the eye which had been removed, Mr. Hulke found that lying in the meshes of the delicate connective tissue, between the outer fibrous sheath of the nerve and the mass of nerve-filaments, were small clusters of cancer-cells, and these were traced up to the point of section of the nerve. Of course they did not end there, but extended into the nerve beyond the point of section; and hence the speedy return. The nerve, be it remarked, appeared to the naked eye perfectly healthy.

If now we look to clinical cases, we constantly find evidence of the fact of this travelling of the cancer elements to a distance

from the parent tumour. A very common phase of cancer is this:—A tumour is seated in the mammary gland, and the skin becomes adherent and puckered. The tumour does not tend to grow faster than is usual; but at some distance from it, from half an inch to three or four inches, small hard tubercles will be developed in the skin. There may be only one or two at first. The surrounding skin will appear to be soft and natural. By and by more will appear, and will perhaps coalesce and form broad hard plates. No surgeon would ever think of operating when these outlying tubercles have appeared. He would know that the disease would return at once in the seat of operation.

It may be said that I am begging the question in taking this fact as illustrating the dissemination of cancer from a parent stock. When, however, it is considered that the fact of dispersion is proved in the dead-house and by the microscope, that primary scirrhus of the skin is extremely rare, that here we have it developed in a number of separate nodules around an original tumour, I imagine that no one would be disposed to deny that these outgrowths were from germs thrown off from that original tumour.

We have, then, three channels through which cancer may diffuse itself—the lymphatic system, the vascular system, and the interstitial system of the various tissues; and through one or other of these channels we may conceive that any part of the body may be sooner or later infected by the migration of some of the cancer elements. Even admitting this, however, there are difficulties in the way of accepting the doctrine that cancer is at one period of its life a purely local disease. How account for the simultaneous development of cancerous growths in various parts of the body? Why the almost constant return after removal? Why should an operation give immunity for years, and then the disease return? Why should cancer sometimes retrograde and disappear, especially when another disease is making progress?

The simultaneous development of multiple cancerous tumours has been already alluded to. Evidence is wanting of the existence of such a case. Indeed it is scarcely capable of proof; for how can it be shown that in no part of the body was there a tumour existing before these multiple ones appeared? The rapidity with which melanosis and encephaloid will be disseminated, when once a tumour has formed, is well known. I have mentioned a case in which, on the removal of a solitary encephaloid tumour on the sole of the foot, a host of similar tumours were rapidly developed in the whole limb and lymphatic glands. Until some undeniable case of the multiple simultaneous development of primary-cancer can be produced,

it is needless to argue the point; more especially as, even then, it would be no conclusive proof of blood-poison, any more than would a simultaneous eruption of warts.

The next question is—why the almost constant return after removal? The conditions of cancer, viewed by the light of recurrent disease, give us a solution. Enchondroma, fibroma, &c., we find, return after removal in distant parts. We have the evidence that these structures may be carried by the lymphatics or blood-vessels, and when arrested at any point may grow. If this may occur in the case of structures so coarse (if one may use the expression) as those named, how much more likely would be its occurrence in connection with a structure the very essence of which is absence of coherence of its elements. It may be illustrated by the difference we see when a recurrent non-cancerous growth and a true cancer are cut into. In the former case, in order to get at any of its component parts for microscopic purposes, we must cut off or scrape up a portion of the tissue. In the latter, the cells, which constitute the active part of the cancer, exude on the cut surface, which presents a creamy character. Nothing is more easy to conceive than that the elements which compose this creamy matter, lying free and without limitary membrane, in the midst of soft tissues, would be taken up by them, by their lymphatics or blood-vessels or connective tissue spaces, and be carried anywhere and everywhere through the body. Principally, one would expect to find them within the range of the primary tumour, on the same side of the body; and so, in reality, we do. But there is no real limit. Just as we find that when a paper of pins is swallowed they may course through the body to any extent, upwards or downwards, or in any direction—so, only infinitely more readily, might these minute germs of disease travel to any extent or in any direction. The differences observable in the various forms of cancer offer a support to this view. Take the four forms of rodent cancer, epithelioma, scirrhus, and encephaloid. It is, I am well aware, a disputed point whether rodent ulcer is really cancerous. I believe it to be so, as the structure of the deposit around the ulcer is like that of epithelioma, and on account of its strong tendency to recurrence. Its elements are, however, very localized—it does not travel usually even to the neighbouring glands. Epithelioma, of which the elements are grosser and more coherent than those of the higher forms, will affect the neighbouring glands, but will not readily pass beyond them, though it *may* do so, and give rise to secondary growths, just as will scirrhus. Scirrhus readily affects the glands, and will contaminate distant parts, but with nothing like the rapidity that is often seen in encephaloid, the structure

of which is far softer, and the fluid more abundant, and laden with minute elements, nuclear and granular; yet it is found that of all the true cancerous diseases encephaloid is the one which may go on to spontaneous cure or to long quiescence. This would appear to militate against the view that cancers contaminate in proportion to the fluidity of their contents. But an explanation is found in this: encephaloid is often the least infiltrating of cancers; it pushes the tissues aside, it forms connective-tissue capsules around it, and hence in a large number of cases it is not brought into connection with the tissues, as scirrhus is, so as to be disseminated amongst them. And this very fact that the most malignant of cancers, the most rapidly growing, and the most destructive when it is disseminated, is often encapsuled, often non-infiltrating, and often less venomous than other forms, confirms the notion that it is not to constitutional but to local conditions that we must attribute the malignancy of the disease.

The laxity of tissues and the activity of vital action going on in them exercise a marked influence on the dissemination of cancer. In young persons—in those in whom there is much fat—tumours grow more rapidly, and are more quickly and widely disseminated. I have removed cancers from the midst of abundant fat, where minute points of disease have been traceable in all directions around, to the distance of from two to three inches. Surgeons know that they cannot take the fat away from around a cancerous breast too widely. Surely no one would consider these as evidences of blood-disease setting up cancers *de novo*, considering that the fat is not a tissue in which primary cancer is formed.

[Mr. De Morgan, from careful inquiry into the histories of private patients, is inclined to consider the disease as more frequently hereditary than does Mr. Paget.]

I should place cancer, then, at the top of a scale, at the lowest point of which might be placed the simplest forms of outgrowth identical in structure to the parts amongst which, or from which, they grow. In all cases I should regard the morbid product as the result of undirected or ill-directed growth force, the elements of the structure thus formed possessing, as the natural elements do, the tendency to produce their like. Consequently, I should consider that a special condition of blood had no more to do with the formation of a cancer than it had to do with a lipoma or an adenoid.

It may be asked, what is practically gained supposing these views are established? Little or nothing, I admit, with our present knowledge, except this, that we should know the direction in which we should work. In the first place, it is

clear that if removal be the practice adopted—and I believe that in all fair cases it is the proper one—the earlier the operation is done the better. This I consider a positive rule. It is true that very early operations are often very unsuccessful, and late ones very successful. But the tumour itself will often tell us why. I have removed tumours from the breast when small, and only just discovered, and have found them highly vascular and juicy, and the tissue around containing minute dark specks of cancer. Such tumours would be very likely to return. Others, which have existed a long time, are hard, circumscribed, contracted, with no glandular infection. In such cases the most satisfactory results might be expected from operation, provided always the operation is widely performed. In cases like those first mentioned the operation might be successful, if the parts around were as extensively removed as they would be in the case of a large tumour existing. I believe I may state from my own experience that the most successful results of removal have followed early and wide operations. Even when the axillary glands are affected, and are removed freely, the patients have lived for many years without return of the disease, as they will at times when enlarged glands are left untouched. And I have no doubt that the use of agents which coagulate the albumen to some depth, and at the same time, or in consequence, act as antiseptics, are beneficial. I still use the chloride of zinc, as I know of none which fulfils these conditions better.

The hypothesis of the local nature of cancer precludes the notion of general treatment with a view to alter the condition which produces it. I do not doubt that by general as by local means an impression may be made at times on the rate of progress; but I am quite sure that no system of internal or external treatment hitherto discovered has the slightest *special* effect. And I do not say this without experience; for, in the hopeless state we are in with regard to the treatment of the disease, I have felt it my duty to try many of the remedies which have from time to time been lauded. They have all done good in one way: they have made the patient hopeful for a time, and that is something. But, so far as any real effect on the disease was concerned, pump-water would have been as useful. One mode of treatment has occurred to me; but I have not had the courage to try it, because I could not answer for the results. They might be unfavourable. Cancer is for the most part, and at its commencement, a disease of the healthy and well nourished. As a rule, I believe that it is best in the early stages rather to keep the patient on a restricted diet than to encourage them to take largely of food and stimulants. They have less pain; and the tumour is, I believe, less active. Could

the disease be starved out? Many years ago a gentleman of strong health and iron will told me that he suffered severely from hemorrhoids. He disliked the idea of operation: and was told by his medical man that if he would starve himself he might obtain a cure. For weeks he kept himself at starvation-point, or nearly so, and the tumours all became absorbed; nor has he been troubled with them since. Hemorrhoids are not cancers; but the example shows that when the body is feeding on itself it does not spare adventitious growths. One of the most distinguished in this country, since dead, told me that his wife had had cancer of the uterus. He kept her for a length of time on the sparest vegetable diet—just enough to keep body and soul together. The disease disappeared. Years after the cancer reappeared, and destroyed her; but they were then living separate. May the antagonism occasionally observed between tuberculosis and cancer be dependent on the fact that tuberculosis is associated with waste of tissue? These are the slightest possible grounds on which to suggest a mode of treatment which might prove injurious instead of beneficial. Still I should like to see the experiment tried. It might be made in the case of melanosis in the horse, though I do not know whether that is a true cancer. Still the effect on an abnormal recurrent growth would be worth noting. In order to be efficient it must be tried in a healthy animal, before any general effect on the system is produced by the disease.—*Lancet, July* 1, 8, 15, *and* 29, 1871, *pp.* 6, 41, 80, 155.

8.—ON FUNCTIONAL MEDICINE.

By Dr. Willoughby F. Wade, B.A., Physician to the General Hospital, formerly Professor of the Practice of Physic in the Queen's College, Birmingham.

In practical medicine, as in morals, it is well to have a lofty ideal. If we aim at a high mark we shall shoot higher than if we aimed at a low one. But in both there is a danger in having too lofty an ideal. We feel so utterly unable to reach that which we profess to ourselves we ought to reach that we do not long continue to aim at it. This is the case with many young practitioners of medicine. They set out impressed with the advantages of minute investigation, of accurate research, and of exhaustive diagnosis by those various means and instruments which they see used in hospitals. They find afterwards, when pressed by the cares and fatigue of a large practice, that they are almost compelled to forego their use; they find also that in many cases they get on pretty well without them, and thus fall into the habit of neglecting them, even in those cases where their use is, for purely practical reasons

imperative. They thus in course of time dissociate themselves from scientific medicine, which they come at last to despise. That this conclusion is actually arrived at by a large number of practitioners, many of whom do not hesitate to avow it, I appeal to the experience of every one; and I think I am not wrong in the genesis to which I have attributed it. I have long been convinced that this state of things cannot be rectified by the earnest and genuine exhortations of teachers. If it could have been, it would never have existed. It is the result of too high an ideal and the inexorable logic of circumstances.

The ideas or principles which may, I think, eventually lead to an alteration of this unsatisfactory condition may be summed up in the term "Functional Medicine." In explanation of this term, and of the ideas involved in it, I request your attention to the following considerations. What I mean by it is this: that whenever we come to treat a case, to prescribe drugs or particular diets, rest or action, we should first of all consider what function of the body it is that is improperly performed. To the setting right of that function we should address ourselves. It may be, and indeed generally is the case that more than one function is (it may be several are) astray. We have, then, further to consider whether it is possible or convenient to attempt to rectify all these at once; and, if not, we have to decide which we should begin with.

Now this method has several recommendations. Amongst the first is, that it is not too high an ideal to be constantly aimed at. In a large number of cases it is closely allied to the principle which, as I have told you, many persons act upon—namely, the treatment of symptoms. Because every symptom of disease arises from the imperfect discharge of some function by its appropriate organ. Hence it requires only a slightly higher order of thought than that which is commonly in vogue. Indeed, many persons who profess only to treat symptoms do rather aim at the treatment of function. They misdescribe their principles, and so do themselves an injustice. Now, though this admission may indicate that the principle I am laying down is comparatively wanting in novelty, it is strong testimony to its practical utility. Practice would be much more imperfect than it is if all those who profess to treat symptoms really contented themselves with doing so. For this reason: if we treat a symptom merely, we often fail to remove that which causes the symptom. To give an extreme instance: If a man has a thorn in his hand, and we merely order opiates for the relief of the pain thus produced, we are very inadequately treating the disease, though we may be adequately treating the symptom. In some cases treatment based upon such a principle may actually aggravate the disease. You

will remember that in my last lecture, which was upon certain urinary deposits, I pointed out to you that lithiuria sometimes produces impotency; and I mentioned to you that persons thus suffering, in whom the cause of the disorder had not been recognised, may, and are advised to, take tonics and live freely, both of which, and particularly the latter, have a direct tendency to aggravate the lithiuria, and so to increase the disorder they were intended to cure. Treatment of this kind is directed to the symptom—impotence,—because in some such cases depending upon other causes this treatment may be beneficial. If, then, every person who now professes to proceed upon the plan of treating symptoms would in each case profess to proceed upon the plan of treating functions, there would be a great gain to practical medicine. Miscarriages of treatment of the class I have pointed out—and they are very numerous indeed—would be avoided. Further, this principle would bridge over the lamentable chasm which now divides the science and art of medicine. Accomplished pathologists and morbid anatomists, whose true knowledge of disease has justly led them to recognise the incurability of many organic diseases, would enhance the benefits they have conferred on mankind if they would condescend to rectify those functions of which the apparatus is not totally disorganised, and thus much misery would be averted, and many lives eased and prolonged. On the other hand, those who find by experience the limited control which we can exercise even over functions, and the extreme difficulty with which they can in some instances be influenced, would gladly hail any assistance which science might afford them by its most recondite researches, when these throw any light upon the laws which govern the growth of cells, the distribution of the blood, or other mysterious acts upon which the discharge of function depends.

Another advantage is this: persons who got as far as I suggest would get further. It is impossible that any of you who have paid attention to cases in the hospital, and have watched or assisted in their investigation, can have failed to be struck by the fact that one and the same disorder of a particular function may in different cases arise from very different causes. Take, for example, epileptic fits. You have doubtless seen them, in the surgical wards, caused by external injury; in the medical wards, you have seen them dependent upon the presence of worms in one case, upon disorder of the stomach in another case, and upon other causes in other cases. Now, knowing this, and resolving to treat not merely a symptom, which in the case of epilepsy is usually done by a miscellany of reputed specifics, you would naturally be led to examine into the case closely, and discover, if you could, the function

which was permanently disordered, and the cure of which disorder (often lying very deeply hidden) would cure the more apparent and prominent phenomena, of which only the patient complains. Many cases of epilepsy may in this way be relieved. And remember this, that any specific must influence some function, though we may not know what function is disordered, still less the organ by which it is performed. In illustration of this, take the case of potassic iodide and secondary syphilis. A man has syphilis, and some time afterwards skin eruption, or sore-throat, or falling of the hair, or painful swellings, or enlarged glands, or albuminuria, or anæmia, or iritis, or dry pleurisy, or simple cachexia, or a combination of these symptoms. We give him potassic iodide. The symptoms disappear, and there is no evidence that his health is not as perfect as in his pre-syphilitic period. Twelvemonths after, some of these symptoms return. What do we infer from this? We assume that these symptoms arise from a poison in the system. If so, there must be some organ which manufactures this poison, and for a season ceased to do so; or else there must have been some organ which for a period ejected the poison from the system, and which after a time ceased to do so; some organ, therefore, the peculiar function of which is deranged. What this organ is we know not—still less in what way its healthy action is modified. But the effect of the iodic salt has undoubtedly been to restore that function to its normal condition. So you see that Functional Medicine perfectly well admits of the employment of such a specific, though the discovery of its powers may have been purely accidental and its *methodus medendi* be still unknown. Examples of the alteration of one function by the disorder of another might be multiplied almost infinitely. Take bronchitis only. Upon what an immense variety of causes, direct and indirect, may it not depend. Its treatment may or may not be influenced by our knowledge of its distant origin. It does not at all follow of necessity that the physician who treats it with simple expectorants, after, by minute inquiries, discovering many complications, is in error, though you may have thought so from my remarks in an early portion of this lecture. You see now that, in my opinion, Functional Medicine incorporates all the benefits of the imperfect method of symptom-treatment, and moreover leads directly on to the recognition and employment of the most precise, searching, minute, and scientific methods.

Another great recommendation of Functional Medicine, in my eyes, is, that it explains the necessity, and thereby justifies the use, of empirical plans and remedies. I will explain what I mean by empiricism. We seek to modify the function of the kidney. We have but a limited insight into the working of this organ;

we know that it consists of certain parts, and we know to some extent the working of these parts. But we know very imperfectly. We find that in a state of health the quantity of the urinary secretion may be increased in numberless ways. When we come to use some of these means in a case of dropsy, where hyperuresis would be useful, they totally fail. We cannot tell why this is. Or we find that one amongst many succeeds. We can no more tell why this is. We only know the fact. Empiricism recognises this imperfection of our knowledge; and it also tells us how best to counteract it. Do not persist in the use of a drug which does no good, or even possibly does harm, because, judging from some data or others, we have come to the conclusion that it *ought* to do good; on the other hand, do not cast aside drugs or methods which seem to do good, or have done good, in apparently similar cases, because, judging from some data or others, they ought not to do good. Many proofs might be given that such a warning as this, superfluous as it may appear, is not really so. I will give but one. Dr. Bright, who was the chief discoverer of the diseases which go by his name, was in the habit of using diuretics in the treatment of them. I have always done so, and in many cases with signal success. Yet, in spite of this teaching by empiricism, their use has been strongly deprecated, because, judging by certain microscopical appearances, diuretics ought to be injurious. In treating a case of Bright's disease, I try to restore the function which is most seriously disordered. I find that in many cases, not in all, the only way to restore this function to something like its proper performance is to use diuretics.

Functional Medicine leads on a little further in another direction. It leads us to anticipate disease, and so to forestall the enemy. Thus we find that in the course of certain diseases there is special liability to interference with some particular function, as in the case of the kidneys in diphtheria. Precautions may in this instance be readily taken which, though perhaps not infallibly, will probably prevent the functions of the kidneys being seriously interrupted, and thus we may prevent the very dangerous consequences which this interruption entails. We can similarly prevent scarlatinal dropsy.

Functional Medicine also justifies, and may therefore restore, some old and wise practices which have been submerged under the advancing wave of modern science. When it was not well recognised that many diseases tended naturally to cure themselves, methods were adopted for their relief which are now disused, not because they have been proved to be wrong in practice, but because they became unnecessary in theory. For

example, the use of a sharp calomel purge at the outset of the exanthemata, once universal, is now rarely resorted to; yet the beneficial effects of this plan in ordinary cases need not be insisted on to those who in their own proper persons have been so treated. But where we find, from our knowledge of the natural history of a disease, that there is a tendency to pernicious diarrhœa, as in typhoid, then we should either refrain from such treatment or use it with extreme caution. This is another example of anticipation in Functional Medicine. Speaking of typhoid calls to my mind another example of the justification of an old treatment by Functional Medicine. I have never seen any more decided benefit produced by any treatment than I have seen, years ago in the Paris hospitals, from the application of leeches or cupping over the cæcum in certain cases of typhoid. Yet typhoid is a disease of debility, and we are urged nowadays in cases of debility to ignore all special functions and "restore" the patient. How, then, are we to explain the success of this anomalous treatment? Very simply. The cupping restored the function of that part of the mucous membrane the disorder of which, in the particular cases referred to, had aggravated the general disorder of the system. In the weakest patient it is often better to restore a function the irregularity of which is keeping the patient back, than to try to force them into strength by stuffing them with food and tonics and stimulants. How often do we see an enfeebled and disordered stomach thus overburdened and exasperated, instead of soothed by appropriate diet and quieting medicines.

The last consideration which I shall offer you as evidence of the propriety of the ideas which I have summed up in the term Functional Medicine may not be at first sight one of great practical weight. It is, nevertheless, in my estimation, anything but trivial. It is that this principle of action is in harmony with the latest and highest conceptions of philosophical medicine. To you who have probably not been much accustomed to abstract or metaphysical considerations, I may perhaps find a little difficulty in conveying my meaning. I will, however, try to do so in a plain and simple manner. What is disease? What do we mean by the term? In old times people used to think that a disease was some actual entity or thing which had got into the body in some way, and was there lying hidden and secreted, and was to be cast out. We know that there are a few instances of this kind of thing. For example, we know that some disorders depend upon the presence of a tapeworm in the intestines, and that to cure them we must hunt the worm out of the body. This idea, which we now know to be true only in a few specific instances, was at one time general. The germ of this idea still runs through a great many of our con-

ceptions of the nature of disease. This is more particularly the case in the pathological conceptions of ill-educated laymen. But many traces of it are still to be found amongst ourselves also. It is not long since the most illustrious and philosophical of existing pathologists has been dealing with this question; and the conclusion he has come to is that all disease is disordered function. Here, then, I say, is the highest justification for all treatment being based upon the principle of restoring disordered functions to order, and this it is which I have ventured to term Functional Medicine.—*Lancet, July* 1, 1871, *p.* 4.

9.—THE SECRETIONS AS GUIDES TO TREATMENT.

By Dr. H. W. Fuller, Physician to St. George's Hospital, London.

[Failure of treatment frequently depends upon a neglect to examine carefully the character of the secretions. Our diagnosis of the precise nature of a disorder may be perfect, but this alone does not always suffice for success in its treatment.]

Let me take as an example the case of a man at present in the Cambridge ward, who was admitted suffering from anasarca, occasioned by a weak and dilated heart. In addition to extensive dropsy of the extremities, he had urgent dyspnœa with orthopnœa, congestion of the lungs, effusion into the pleural cavity, engorgement of the liver, with pale-coloured fæcal evacuations, and scanty, loaded, non-albuminous urine, tympanitic distension of the abdomen, and slight effusion into the abdominal cavity—the ordinary assemblage of symptoms which result from long-continued interference with the central organ of the circulation. Before I saw him he had taken salines with digitalis, squills, scoparium, and other diuretics; but his urine had not increased in quantity, and his symptoms had been gradually increasing in severity. This was his condition when he was admitted into the hospital. Having regard to the pale colour of his motions, I felt convinced that no real good could be effected unless a free flow of bile could be induced; and that the failure of the treatment he had undergone before admission into the hospital depended principally, if not solely, upon the neglect to stimulate the action of the liver and the secreting apparatus of the bowels. I therefore prescribed five grains of the compound digitalis pill—containing three grains of blue pill, a grain of squills, and half a grain of digitalis—three times a day; and within three days he had begun to improve. Before the end of a week the motions had assumed a healthy colour, the quantity of urine was trebled, the flatulence had almost disappeared, the ana-

sarca was rapidly decreasing, and the dyspnœa subsiding. Indeed, it was obvious that the very medicines which before had proved inoperative had become active agents for good as soon as the secreting apparatus of the liver and bowels had been stimulated to healthy action.

Take another class of cases. It sometimes happens that patients are admitted suffering from ague, with which they have been afflicted for many weeks. They have taken quinine perseveringly, but have not obtained relief. On examination the case is at once apparent. Their internal organs are in a state of engorgement, and consequently are sluggish; their bowels are costive, and their motions unhealthy. In these cases, the administration of remedies calculated to stimulate the viscera to action is all that is needed to restore them to health. Two or three doses of colocynth and rhubarb in combination with quinine will at once arrest the disease, which weeks of mere quinine taking had failed even to control.

In many forms of dyspepsia the same holds good. The secretions of the stomach and bowels are disordered, the liver is gorged, and the tongue is covered with a yellow fur. The patient has been purged by means of senna, colocynth, or rhubarb; alkalies and alkaline earths, together with vegetable bitters, have been given; possibly the mineral acids have also been tried; moderately strict dieting has been had recourse to; and other expedients have been adopted, but in vain. The disagreeable taste in the mouth, the acidity, waterbrash, flatulence, drowsiness after meals, and restlessness at night, continue unabated. And what is the cause of this failure of treatment? Though the bowels act regularly, the motions are pale and lumpy, or else dark-coloured and offensive; and the urine is scanty, high-coloured, and loaded with lithates. Cases such as these often come before us in the wards, and occur still more frequently in private consulting practice. They convey a lesson which must not be forgotten, even though healthy. Dogs with artificially-made biliary fistulæ do not appear to secrete an increased quantity of bile under the influence of moderate doses of calomel. The lesson which they teach, and which you will do well to remember, is, that so long as the secretory apparatus is inactive or out of order, the remedies which are ordinarily most efficacious in relieving the symptoms of dyspepsia are of little avail; whereas they exert their beneficial influence directly that disorder is rectified, and secretion is re-established. In cases such as these, a few doses of calomel, combined with opium if necessary, will in a few days effect a change for the better, which cannot be brought about by other means in as many weeks. Under their influence the motions will lose their offensive odour, and assume a

healthy colour, the tongue will clean, the urine will become clear, the symptoms of acidity and discomfort will pass off; and your patient will give you credit for affording him relief. When this healthy condition of the secretions has been attained, you need have little anxiety as to your patient's recovery, for the remedies which had previously proved ineffective will speedily quiet the irritability of the stomach, increase its tone, and restore your patient to health.

Let me take yet one more class of cases—the most common perhaps with which we have to do, and that in which, perhaps more than in any other, a want of proper regard to the secretions leads inevitably to unsuccessful practice. I refer to cases of so-called debility, in which stimulants, high feeding, and tonics are constantly recommnded, and are often fruitlessly had recourse to. The cases to which these observations apply are of every imaginable description. In private practice it often happens that a person, not otherwise out of health, is accidentally deprived of his usual exercise; and, his appetite being unimpaired, he takes more food than is absolutely required for the repair of his body. The wear and tear of his tissues being much less than usual, owing to his inactivity, and the supply of fresh materials, owing to his unimpaired appetite, being in excess of the actual requirements of the body, one of two things must necessarily happen: either his excretory organs must do an unusual amount of work, and throw out of the system the whole of the matters which have been taken in excess of the actual requirements of the tissues, or the surplus materials must accumulate in the blood, alter its quality, and oppress the nervous system. The alternative is not doubtful. When the blood is surcharged with materials which, however good and nutritious, are yet in excess of the requirements of nutrition, the nervous centres are oppressed; and not only does languor, or general debility, as it is termed, occur, but the liver, kidneys, and other secretory organs become gorged and sluggish, the urine becomes scanty, and the motions become clay-coloured, or else dark and offensive. In private practice, cases such as these are constantly met with, in which, notwithstanding the unhealthy state of the secretions, tonics have been given for months, together with stimulants and every variety of strong food, and in which a few days of active purgation, some alterative doses of mercury, enforced exercise, and a restricted diet, by leading to the elimination of the surplus materials, and so to a purification of the blood, do more than all the previous tonics and rich feeding to put an end to the patient's languor, and restore his physical and mental power. In hospital practice, these particular cases less frequently come before us; but in another form, and under other

circumstances, similar instances not unfrequently present themselves. In the Roseberry ward, at the present time, is a stout, strong-looking, hysterical servant girl, Sarah Fleetwood, suffering from amenorrhœa. She had undergone treatment for a considerable period before admission, and throughout had taken iron in various forms, but had not obtained relief. On admission, she complained of extreme languor and debility, and of utter loss of appetite. Her tongue was clean, and her bowels were said to be regular; but her urine was scanty and loaded with lithates; on examination her motions proved to be very scanty, and almost white, lumpy, and offensive, and had been so, according to her account, throughout her illness. She is a perfect type of the class of cases which I have been endeavouring to describe. She is oppressed by the presence in the blood of matters which ought long since to have been eliminated, but which the inactivity of her secretory organs has caused to be retained in the system; weak, in the sense in which a healthy person on the eve of a bilious attack is weak, but in no other; weak from a cause which no strengthening food and no tonics will remove, nay, rather which they will tend to aggravate, and which protracted semi-starvation or the skilled aid of the physician is required to rectify. The iron and other tonics, the port wine and other stimulants, which the poor girl had taken prior to her admission into the hospital, only tended still farther to surcharge her system with materials which it was incapable of assimilating—to render her blood more noxious to the brain and other nervous centres, and thus to increase her languor and aggravate her suffering. Yet these very remedies have nearly effected her cure, now that healthy secretion has been re-established. Already, under the influence of a few doses of calomel and colocynth, the urine, instead of being scanty and loaded, has become abundant, and in every respect healthy; the motions are no longer pale and lumpy, but have become normal in character; colour has returned to her lips; she sleeps more quietly, feels stronger and less languid; and no longer exhibits a repugnance to food. And now that the iron is able to do its work, its beneficial influence will soon be manifested still more decidedly, and the menstrual discharge will speedily return. In a precisely similar case which I saw with Sir Charles Locock many years ago, the symptoms did not yield until after a protracted course of calomel; and experience justifies me in warning you that in several forms of amenorrhœa, and in many of the so-called cases of general debility, a deranged condition of the alvine secretions lies at the very root of the symptoms, and that all remedies will fail to relieve the patient until steps have been taken to rectify them. Purgation may

or may not be necessary, and the aid of calomel may or may not be needed; but if either or both prove requisite to attain the desired object, they must be used without hesitation. No theoretical considerations must be permitted to countervail such clear and unmistakable indications for treatment. Experience confirms what theoretical considerations would lead us to expect, namely, that the true mode of restoring strength, under the conditions we are now considering, is, not by administering food and tonics which the patient is incapable of assimilating and making use of; nor is it by abstaining from administering alterative or aperient remedies, lest by so doing we should weaken the patient—both of which courses can only tend to continued mal-nutrition and a gradually-increasing failure of strength—but rather to endeavour, by appropriate means, and at whatever risk of present discomfort to the patient, to re-establish healthy secretion; and thus place him in a position to profit by wholesome food, the natural restorative of health and strength.—*St. George's Hospital Reports, vol. 5, p.* 17.

10.—THE APPLICATIONS OF ELECTRICITY TO MEDICINE.

Electricity, as used in Medicine, is either static (derived from the friction of a glass-plate or cylinder), or dynamic (the direct product of chemical action), or induced from the contact-current by an induction-coil. Static electricity is not much employed. Contact-electricity or constant-current galvanism is of much greater importance; but its value is only now becoming fully understood.

In a former article on this subject (*vide* p. 611, vol. i., 1871), we discussed these two varieties of electricity—viz., Frictional, or Franklinic Electricity, and Current, or Galvanic Electricity—and we considered them with special reference to the modern doctrines of force or energy. Putting on one side all further consideration of electricity as related to other forces, we shall next proceed to discuss that form of electric action which was discovered by our countryman Faraday, and which is commonly known as Induced, or Faradic Electricity.

The induced electricity (the interrupted current, or faradisation) is that most commonly employed, and is derived either from electro-voltaic or electro-magnetic instruments. In the former, the electricity is primarily developed by chemical action; in the second, by the rotation of a kind of wheel.

Suppose we construct a coil of copper wire, covered with silk to insulate it. This wire had better be rather thick, and

number of turns need not be great, especially if wound round a core composed of soft-iron wires. If now we construct another, also of copper wire, finer in character, and made up of a great number of coils, but built so as to constitute a cylinder, into which the former may be introduced, we have an induction apparatus. All that is now necessary is to connect the extremities of the thick wire with a battery, and the extremities of the thin wire with some object capable of testing the existence of electrical effects, when it will be found that the moment the circuit through the thick wire is closed the poles of the thin wire also give evidence of a galvanic current, stronger than that produced by the battery and passing in the opposite direction—this, too, without any direct communication between the contained and containing coils, the former of which is called the primary, the other the secondary. But when the current has been fairly established in the primary coil, its effects on the secondary cease until the former is arrested, when signs of a current, this time in the direction of the primary one, are again manifested by the secondary coil. Thus, in induced electricity the current is to and fro, and can only be produced by breaking or closing the primary current. To keep up its effects, therefore, some apparatus must be introduced, constantly opening and constantly closing this current. This is commonly known as the *trembler*.

But there is another form of induced electricity still more frequently employed. It is called electro-magnetism. Thus, if a bar of soft iron, shaped like a horse-shoe, be surrounded by either extremity of a coil of protected wire, wound in opposite directions, and if through this wire is passed an electric current, the iron bar is converted into a magnet. But suppose something quite the reverse of this—that is to say, a magnet, horseshoe-shaped and powerful—is the origin of the energy, instead of a battery. If, then, we have two bobbins made of copper wire covered with silk, and surrounding soft-iron centres, the two being connected on an axis by a soft-iron plate; if, further, we have the means of causing these to rotate rapidly on this axis, either iron core coming close to both poles of the magnet in the course of its revolution, we have the well-known magneto-electric machine. Here magnetism is transferred with each approximation from the magnet to the soft-iron core; the soft-iron core in its turn induces an electric current in the copper-wire coils, whence by appropriate mechanism it is conveyed to the galvanometer or human body, as the case may be.

These are the two forms of electric force most generally employed in Medicine, chiefly on account of the ease with which they may be produced and applied.

A word next as to the means commonly employed of giving

rise to current electricity, whether for applying it directly to the human system, or for producing an induced current for that purpose. This is attained usually by constructing a series of couples, forming what is known as a galvanic battery. The electricity so produced is, of course, current electricity, and this current is *continuous*. The current in the case of induced electricity, on the other hand, is to and fro, or, as it is also termed, *interrupted*. Now, in batteries constructed on the principles we have laid down—that is to say, made up of electro-positive and electro-negative plates, with a liquid intervening—there is great difficulty to be faced in rendering the current equable. Hence these have now generally fallen into disuse, and batteries with two liquids instead of one are commonly employed. The reason why the galvanic action of the two elements becomes enfeebled is twofold. Thus, in the zinc-copper combination, the sulphuric acid combines with the zinc, rapidly at first, more and more slowly afterwards, as the acid comes nearer and nearer to neutralisation. But this is not the principal difficulty; for, as the sulphate of zinc forms, hydrogen is set free, and this clings most pertinaciously to the inactive copper plate, interfering with the circuit of the current; and not only so, but, by reducing some of the sulphate of zinc, it coats the copper plate with a layer of zinc, and so gradually approximates the character of the two plates.

To remedy this last difficulty a second liquid is introduced, and so placed that the hydrogen set free in forming the sulphate of zinc shall attack it rather than the inactive plate, which in this way is untouched, and thus the strength of the current remains unimpaired, so that the current so produced is known as the *constant* current. One of the best forms of constant-current battery was invented by the late Professor Daniell, and is known by his name. This is a zinc-copper battery, but the copper is immersed in a solution of sulphate of copper. Between this and the dilute sulphuric acid a diaphragm of glazed porcelain is interposed. By this means the two liquids are kept from uniting too speedily, without interfering with the passage of the electric current. When in action the hydrogen attacks the sulphate of copper, reducing the metallic copper which is deposited on the copper plate. The sulphuric acid passing in the opposite direction tends to renew the acid attacking the zinc. Provision is easily made to keep up the supply of sulphate of copper, and so the current remains constant for a very long time.

Some form of this battery is that most useful in Medicine, provided a constant current be desired, but only if the battery is to be stationary. The bulk of the elements and the mass of sulphate of copper crystals requisite, if it is to be used for any

length of time, interfere with its portability. One of the best for the Practitioner, or rather, we should say, for Hospital practice, is constructed by Messrs. Elliott and Co., Strand. This will continue to act steadily for a very long time, but it is bulky, and cannot be readily transferred from place to place. As copper and zinc are not very far removed from each other in the electro-motive series, inventors have sought to increase the electro-motive power of a battery, by selecting as the negative element platinum or carbon. The former is the principle of Grove's, the latter of Bunsen's battery. In either case, nitric acid is used instead of sulphate of copper. This, being attacked by the hydrogen, produces nitrous acid, and so protects the platinum or carbon. These batteries are exceedingly powerful, but the nitrous acid fumes, evolved after a time, are extremely objectionable, and so chromic acid is frequently used as a reducing agent. The battery in most common use nowadays for Medical purposes is constructed on this principle. It is made by Stöhrer, of Dresden, and consists of zinc-carbon elements, with chromic acid as the reducing agent. Two forms are in use—one where one or two of these elements are employed to give rise to an induced current; another where fifteen or twenty are combined, for the purpose of producing a constant continuous current.

In all these batteries two liquids are used, but in Smee's battery a nearly constant current is obtained with one liquid by mechanical means. The elements are zinc-platinum, but the platinum plate is roughened by covering it with spongy platinum, whereby the disingagement of the hydrogen is greatly facilitated, and the resistance is reduced to a minimum. The most useful Medical battery for producing a constant current and, at the same time, retaining portability, is a modification of this, made by Weiss and Son, the well-known instrument makers.—*Med. Times and Gazette, July* 1, 1871, *p.* 16.

11.—OSTEO-ARTHRITIS, OR SO-CALLED RHEUMATIC GOUT; ITS ANALOGIES, NATURAL AFFINITIES, AND ANTAGONISMS.

By Dr. H. W. Fuller, Physician to St. George's Hospital, London.

[A safe means of estimating the true nature of a disorder is to trace the analogies which it offers to other forms of disease, the affinities which it displays, and the antagonism which its existence seems to present to the inroad of other forms of complaint. For instance, there is considerable similarity in the general symptoms between osteo-arthritis, or so-called

rheumatic gout, and scrofulous affections of the joints, there is in both a constitutional taint and the same tendency to the formation of oseous growths around the joint. There are, however, well-marked antagonisms between the two diseases which are both striking and instructive.]

So close is its affinity to phthisis pulmonalis, that out of four hundred and seventy-six patients whose history I have carefully investigated, no less than one hundred and ninety-six, or 40·3 per cent. belonged to decidedly consumptive families, and had lost a father, mother, brother, or sister, from the effects of pulmonary disease. Yet, notwithstanding the strongly-marked hereditary tendency to pulmonary disorder in the families of those who suffer from rheumatic gout, the antagonism to phthisis, which results from the existence of osteo-arthritis, is most remarkable. In the whole course of my experience I have only known five persons die of consumption whose joints have been distorted by osteo-arthritis, and in all those five the active symptoms of the articular disease had long subsided before the pulmonary disorder commenced. In other words, the tendency to osteo-arthritis was replaced by the tendency to phthisis, and did not run on concurrently with it.

The antagonism of osteo-arthritis to albuminuria is even more striking. I examine the urine of every patient who consults me in private practice, and thus can testify to the condition of the urine in many hundred cases of rheumatic gout; but I have never met with an instance of albuminuria associated with this form of articular disease. This is the more remarkable from the fact that albuminuria, to a greater or less extent, is met with in nearly 25 per cent. of all cases of acute disease, and in a considerable proportion of chronic disorders; and may be regarded almost as the natural accompaniment of old cases of chronic gout, in which deposits of urate of soda have taken place in the joints—cases which are too often confounded with osteo-arthritis, in consequence of the distortion of the joints which accompanies the latter form of disease. Indeed the contrast afforded by the condition of the urine in cases of osteo-arthritis and in those of chronic gout, may often serve as a means of diagnosis between the two disorders. In the one the urine is usually of normal specific gravity, and never albuminous; whereas in the other the specific gravity is usually much below the normal standard, the presence of albumen is not uncommon, and when chalk-stones are present, is almost constant.

Thus, then, on reviewing the history of rheumatic gout, it is impossible to avoid the conclusion, that the disease, like phthisis pulmonalis, results from a form of mal-assimilation in

some way connected with a depressed state of vitality. The causes which usually conduce to its inroad, and which invariably aggravate its symptoms, are all of a depressing nature; many of its more characteristic features point unmistakably to a low state of vitality; whilst its close affinity to phthisis, and the peculiar character of the changes which it induces in the joints—changes which approximate, in many respects, to those which result from strumous and malignant disease—serve to indicate a depressed condition of the general health. It is needless to add, that the result of treatment confirms the impression thus derived from the history of the disorder. You are all aware that I never treat it as a form either of gout or rhematism, and do not employ colchicum, alkalies, or other of the so-called specifics for those complaints; inasmuch as they only tend to depress the patient, and hasten the onward progress of his malady. Even in the acute form of the disorder—when the patient is usually bedewed with perspiration, and declares that it is agony to move—I order him to leave the bed, with the view of getting rid of the relaxing influence which tends to keep up the enfeebling perspiration. If that step does not suffice to check the sweating, I prescribe a cold shower-bath or the dripping-sheet, and at the same time administer the mineral acids and vegetable tonics, with cod-liver oil and a generous diet; and, provided the urine remains clear after standing, I do not hesitate to prescribe iron, and to allow a fair quantity of wine, beer, or porter.

As you have all seen the benefit which results from this treatment, it is needless to do more than beg you to be resolute in insisting on its adoption; for the prejudice of mankind is very strong against any treatment in which warmth does not play a conspicuous part; and in private practice all the tact you possess will be required to get your instructions properly carried out. I do not hesitate to say that there are thousands of persons more or less crippled by rheumatic gout who would have been in full possession of their powers of locomotion if a tonic and bracing system of treatment had been adopted when they were first attacked.

The strange antagonism of osteo-arthritis to albuminuria deserves the closest investigation. At present I can do little more than record the fact; but it seems probable that, with the progress of chemical and other methods of investigation, the cause of this antagonism may be discovered, and that light may thus be shed upon the pathology both of albuminuria and osteo-arthritis.—*St. George's Hospital Reports*, 1870, *vol. v., p.* 14.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON HYPERÆSTHESIA.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital.

[Besides the classical diseases of the nervous system there are a number of anomalous cases which scarcely find a place in a regular nomenclature. Many of these belong to the debatable ground where it is difficult to decide whether we have to do with diseases or with symptoms.]

1. In the *cutaneous surface* we very often meet with a condition which betrays itself by no objective change yet discovered, but solely by an abnormal sensitiveness. The tactile faculty of the skin is not really increased; it is not a more efficient organ of touch than it was before, but quite the reverse, as its nerves are incapable of tolerating the necessary contact with bodies presented to them. Such a condition is conveniently termed hyperæsthesia; and this name I shall continue to use in what I have further to say about it. This state of hyperæsthesia is met with very frequently in sufferers of the quasi-hysterical class, both male and female. One poor fellow assured me that his skin had been so sensitive that he could not bear the least touch anywhere; the pressure of a stethoscope in auscultation was intolerable; and the touch of a crinoline-hoop against his leg in a railway carriage caused him terrible pain. These complaints were made quite gravely; and though we can hardly avoid smiling at them, yet we must admit that to the sufferer himself such a state of things was no joke. Think what you would endure yourselves, if all ordinary contact were a cause of continual pain. The reality of such suffering is materially confirmed by the fact that, in some cases of structural disease of the cord, certain parts of the cutaneous surface manifest intense reflex excitability, though ordinary sensibility is much impaired. In a most workmanlike report in the *Transactions of the Clinical Society*, vol i., p. 102, on a case of locomotor ataxy, it is mentioned that an area of skin on the outer side of the right leg, about two inches and a half square, was, and had been for about two years, in a condition of extreme sensitivity to impressions capable of exciting reflex movements—such that the patient had frequently been thrown into convulsions, and had dropped down in the street, from the mere brushing of a woman's dress against this part. Even when he touched this spot himself, either with his hand or his trousers, whilst dressing, slighter convulsive movements often took place. At these

times he felt an odd kind of sensation in the part, which then diffused itself rapidly throughout the body. In such hyperæsthesia as this the sensorium is but little concerned; but there is evidently no improbability that the cerebrum might be the seat of such disorder, as well as the cord. In fact, this is actually the case in many instances of arachnitis. If you object, that I am now speaking of coarse structural lesions, this will not apply to the case of rickets, where the nervous centres are often exempt from evident lesion, and yet the general tenderness is such that the child cries if he be but touched.

The hyperæsthesia which affects the region of the spine, or, more exactly, the line of the spinous processes, needs to be somewhat specially noticed, because it has been much dwelt on, and is often accepted as a sign (with others) of meningitis or myelitis. Brown Séquard speaks decidedly on this point; he says that "it is almost always found that pressure upon the spinous process of the vertebræ (sometimes even a slight one), when made at the upper limit of the inflammation, causes an acute pain" (*Lancet*, July 14th, 1860). Dr. Radcliffe, on the other hand, is inclined to the view that this pain is mostly absent, at any rate, in uncomplicated myelitis. My own experience leads me to look for it as a nearly constant symptom, but not in that degree of acuteness which characterises the neurotic disorder. This, at any rate, is certain: that pain in the back *per se* is no evidence whatever of inflammation being present in any of the component structures, any more than headache is of the cranium and its contents being so affected; and, indeed, the two dysæsthesiæ are in many respects homologous. Spinal hyperæsthesia extends sometimes over the skin on one side, and not at all on the other; sometimes light pressure is painful, and firm relieves; sometimes the latter is the worse. In some cases, I believe, the bones are involved in the neuralgia, without being at all inflamed.

2. The *lingual and buccal mucous membranes* are sometimes the seat of extreme and most distressing hyperæsthesia. The affected parts may appear quite natural, or at most there may be undue redness and insufficient development of epithelium, giving a "beef-steak" appearance, but not at all more than I have often seen in patients who were quite free from this hyperæsthesia. The following case is a good example of this happily infrequent disorder. Mrs. C., aged 73, was seen August 28th, 1866. She had been suffering the last five weeks with her mouth, which was so tender that she could bear nothing in the way of food but lukewarm milk and sopped bread. Cold things gave her pain, as well as hot. The buccal membrane was very red, especially that of the lower lip. The whole surface, including that of the tongue, was extremely tender; but

I saw nowhere any breach of surface, nor anything resembling an ulcer. There were certainly some superficial fissures on the dorsum, but they were no more than often exist without causing any suffering, and they did not appear to involve the corium. The tongue was perfectly clean and moist. Pressure on the mucous membrane did not render a spot pale even for an instant after its removal. The bowels were regular; pulse 84, of good force. The appetite was very good, but she felt quite exhausted from being unable to take food. She got very little sleep, from the distress caused by her mouth. She had suffered much from rheumatism, and felt the changes of the weather very much. My notes extend over more than four months, and show scarce any change to have occurred during the whole of that time; and I am tolerably certain that she continued to suffer in the same way, though less severely, to the close of her life, or till about April 1869. She constantly kept a piece of soft rag in her mouth, which afforded her some solace. On one occasion, I took the temperature of her mouth, and found it as high as 100.4° F. The membrane of the buccal cavity always presented the same appearance of considerable hyperæmia. At one time, a little eczematoid eruption appeared at the margin of the lips, but it never extended internally. The suffering she endured must have been terrible; she described it as that caused by a burning fire, and declared she was enduring a living death. Her temperament was by no means excitable, and I do not think she exaggerated her griefs in the least. Many remedies were used; but none were of material efficacy, except a nightly opiate to procure sleep. The pathological condition was, I believe, one of neuralgia and hyperæsthesia, blended, as sometimes happens, with vasomotor paralysis, which latter produced the hyperæmia. The neurosis may have been of gouty origin.

3. The *throat* is liable to suffer with such disorder as we are considering, which it is important not to confound with common inflammatory, as the treatment required is materially different. The following case is an illustration. Mrs. N., a young-looking, recently married woman, was seen by me on September 8th. She had been suffering four days with swelling and neuralgia of the right side of the face. Her throat had become sore, so that she had much difficulty in swallowing. On the previous day, she could not get down a piece of sopped bread. There was some nausea and vomiting. The throat appeared normal, but with the laryngoscope I could see that the laryngeal membrane was much congested. Her voice, however, was not altered. She had soreness and tenderness at the sides of the neck, lower down on each side than the tonsils. She expectorated a little reddish mucus. She had some diffi-

culty in getting her breath. Her sleep was disturbed; she woke up restless and feverish. She felt very weak. The urine appeared normal. I advised diligent steaming of the throat, sinapisms to the sides of the neck, fifteen grains of chloral at bedtime, and two grains of iodide of potassium and an ounce of effervescing mixture of citrate of potash three times a day. On the 9th, she slept two hours with the chloral, then woke in great uneasiness, and only dozed the rest of the night. At noon, I found her suffering much. The throat was very sore. She had not been relieved at all by the steaming; swallowing was very painful. She had much pain and tenderness in her neck; more, however at the sides of the muscular part than in the larynx, which I could handle and move pretty freely. Her throat felt dry. Tongue coated; pulse 93. I ordered twenty grains of muriate of ammonia with ten minims of tincture of belladonna in water every four hours, hot fomentations to the neck, and aconite liniment. She also had immediately fifteen minims of chlorodyne, which gave her two hours' sleep; and at 9 p.m. I found her decidedly better, though she had a good deal of strange pain, hardly describable, on both sides of her head. The dose of chlorodyne was repeated at 10 p.m., and in the course of the night she had fifteen grains of chloral; but she was in high fever and delirium all night long. The room seemed whirling round; she felt as if she must get up and go out—as if she should fly. She saw numbers of persons in the room, with great heads and little bodies; &c. At 9.30 a.m. on the 10th, when I saw her, her throat was much better; she took some toast. The catamenia had reappeared, which had stopped, after a day of normal continuance, on September 3rd. I ordered a drachm of tincture of cinchona flava four times a day. On the 11th the throat was much better; she had still dysæsthesiæ about the head, and a large patch of urticarious erythema had appeared about each elbow. On the 12th, she was so well as to call on me and take her leave before setting out for the North. She had sweated copiously the preceding night, had a good deal of eruption on the legs and thighs, but made no complaint of her throat. The attendant neuralgia, the peculiar distress, the nocturnal delirium, the urticaria, and the "juvantia," all go to prove the neurotic character of the disorder, and that it was no common sore-throat, but one where the hyperæsthetic greatly predominated over the inflammatory element. The abrupt cessation of the catamenial flow may have had to do with the supervention of the disorder; but I think the most essential factor was influenza.

4. From the throat we descend to the *stomach;* and among the woes of this much-enduring organ we may well expect to find hyperæsthesia. In fact this condition, in a greater or less

degree, is of very frequent occurrence, and constitutes the chief disorder in those cases to which the term "irritative dyspepsia" is appropriate. In many of these hyperæmia and catarrh exist also, but as secondary complications, rather than as primary maladies. The hyperæsthesia, I believe, as a paralytic neurosis, is prone to implicate the vaso-motor nerves and impair the tone of arteries and the retentive power of capillaries. Such disorder may be simple—*i. e.*, dependent on the state of the general nervous system; or it may depend on reflex irritation, as in pregnancy. In some of these states, pain is the more predominant symptom; in others, vomiting; in others, both are present in a marked degree. Andral relates a case where the tongue was natural, and the patient distressed by a very acute hunger; but the lightest food given to her was thrown up by vomiting, or, if it was retained, caused indescribable suffering. The hyperæsthesia was overcome by cold affusion and persevering administration of food. Here the disorder was simple; and such seems to be generally the case, though in some instances, as in the hyperæsthesia of pregnancy, the influence of reflex irritation is evident. Nitrate of silver is a really valuable remedy in cases where pain is distressing; and, as it need only be given for a short time, you run no risk of spoiling your patient's complexion. It may be seconded, however, or replaced, very frequently, by subcutaneous opiates. I have employed them myself with the best effects. Dr. Clifford Allbutt writes strongly in their praise. One of his cases was that of a merchant who regarded himself as a hopeless dyspeptic, dreaded his meals, and especially his nights. He was cured (at least for the time) by one-fifth of a grain of morphia subcutaneously, at 3 p.m., and 10½ p.m., followed by rest, during ten days, with one-fourth of a grain at night, only for ten days more. I prefer myself to make these injections at the epigastrium, but this is not necessary. When the hyperæsthesia expresses itself in immediate vomiting without severe pain, I have great faith in strychnia, administered in doses of one-twentieth of a grain in not more than half an ounce of water. Let me cite the following instance. A slight-made female, aged 22, married four months, subject to indigestion, and weakened by a miscarriage, was brought to me with severe sickness of ten-days' duration, no food having been retained the last three. I gave her, in my room, one-twentieth of a grain of strychnia, and made her lie down. After she had retched a few times ineffectually, she became much better, "did not feel at all sick," and took successively four teaspoonfuls of milk before she left. I saw her again in ten days, when she was greatly better, had only been sick once, and was able to take chicken.

6. That the *tracheal* and *bronchial* mucous surface is liable to hyperæsthesia, cannot be doubted. There are coughs of the most vexatious and obstinate character, which are either unattended with *râles* or expectoration, or where these are out of all proportion insignificant to the severity of the cough. Some of them are of reflex origin, as in Graves's case, where the expulsion of a tænia put an end to a most violent cough, that went on night and day, and resisted all remedies. Some are due to toxic matters in the blood, as is probably the case in pertussis, and in some cases of gout, where, as Graves says, the cough is dry, annoying, and often very obstinate. Some, again, are simple neuroses, as in the case related by Trousseau, of a young lady who was of an hysterical constitution, but was exempt from catamenial irregularity, and from other nerve-disorders, except a continual cough, which proved refractory to all remedies, but yielded immediately to change of air. The treatment which appears to me most rational and satisfactory in pertussis is that recommended by Dr. E. Smith, of producing and maintaining moderate narcosis by means of small doses of morphia; and this is quite accordant with the view that the paroxysms depend upon a specific toxic hyperæsthesia. There is another form of bronchial hyperæsthesia with which it is very important for you to be acquainted—viz., that which occurs in certain instances of pneumonia, where after a time the respiration becomes inordinately frequent, perhaps 80 in the minute, without any corresponding increase in the pulmonary lesions or lividity of face. In these cases, the hurry of respiration is a mere neurosis, and yields to opium and a due amount of support.

7. The *heart* enjoys no exemption from hyperæsthesia. Cases are not at all rare where there is more or less præcordial uneasiness or pain, with rapid, vivid action of the heart, unduly forcible impulse against the chest-wall, and loud sharp sounds. The radial pulse may be small and weak, contrasting strongly with the apparently forcible efforts of the heart. Such diseases are often seen in young persons, and are sometimes mistaken for instances of cardiac hypertrophy; from which, however, you may easily distinguish them by observing that the dulness-area is not increased, and the apex-beat not lowered nor displaced to the left. Toxic agents, such as tea and tobacco, reflex irritations, as of the gastric mucous surface, and primary failure or disorder of nerve-force, may all be incriminated in different instances as efficient causes. In the latter group, it seems most probable that the nervous filaments distributed to the endocardium are unduly impressible, and, being thus over-stimulated by the moving blood, keep the muscular fibres in a state of rapid action. Anæmia, though a very common cause, is by no means necessarily present. In Da Costa's account of the

curious cardiac malady frequent among American soldiers in the late war, he mentions, as symptoms, constantly recurring attacks of palpitation and pain, whose chief seat is near the apex of the heart, and combined very commonly with excessive cutaneous sensibility. In some of his cases, the disorder supervened on fatiguing marches; in others, it occurred after fevers or diarrhœa. He tells us in his recent paper (*American Journal of the American Sciences*, Jan. 1871), that digitalis or digitaline had more influence on the cardiac disorder than any other drug resorted to. Belladonna internally he also found a most efficient agent, particularly valuable in instances of irregular action. Aconite reduced the force of the heart, and lessened the tension of the pulse; it was most suited to cases of commencing hypertrophy. No special commendation is bestowed on other sedatives; and gelsemin, which is reputed to be a cardiac sedative, disappointed him much; for the most part, it had no effect at all. Morphia hypodermically relieved the cardiac pain almost invariably, and so did atropia; but the effect was only temporary. Tonics were often most excellent adjuncts to treatment, but availed little by themselves. Zinc and iron accomplished most. Da Costa's experience has convinced him that there is a real connexion between the functional disorder, cardiac irritability, and the organic one, cardiac hypertrophy; that, in fact, "the one grew out of the other." Dr. Walshe admitted the same connection several years ago. I cannot refuse to admit the doctrine held by these high authorities; but my own experience would not lead me to rate this cause of hypertrophy highly. I am sure that a weak and irritable heart may simulate very much a condition of hypertrophy, and yet, under suitable treatment, subside into a cond.tion appearently healthy. A belladonna plaster is a remedy that generally does good service, and is to be preferred I think, in most cases, to the subcutaneous opiate, which, in two cases where I tried it, produced more syncope than was at all agreeable to witness. Dr. Clifford Allbutt, however, assures us that subcutaneous injection of morphia in small doses is very valuable in so-called irritable heart, whether this be due to weakness of the organ or instability of its nerves. The first dose administered in this way should not exceed one-tenth of a grain.

8. A sort of diarrhœa, which I have occasionally met with in elderly persons, seems to me attributable with much probability to hyperæsthesia of the *mucous lining of the rectum.* The fæces appear natural enough in all respects; but the lower bowel is intolerant of their presence, and, in a fidgety petulant manner, insists on expelling them much more frequently than is at all convenient. Belladonna has appeared to me of some service in this condition of things, and still more, perhaps, cold sponging of the anus directly after defæcation.

9. Nocturnal incontinence of urine is an instance of hyperæsthesia affecting the *bladder*, to which it may be sufficient to allude. Let me, however, add that recently Dr. Barclay has praised syrup of iodide of iron as unfailing in this disorder. Belladonna in gradually increasing doses, you know, is the most approved remedy; but there is no reason why both should not be employed. A tonic will sometimes quell hyperæsthesia better than a sedative. Hyperæsthesia of the *urethral lining* is a marked feature in many cases of spermatorrhœa, and its removal by means of repeated catheterisation is reputed beneficial.

10. Hyperæsthesia of the *testis* is a very distressing neurosis; it reacts very injuriously upon the mind, and has in several instances induced sufferers to insist on the removal of the affected organ, which in three cases was found healthy by Sir A. Cooper. He considers it of centric origin; but it may be of reflex peripheral origin, as in the following case. A. C., aged 23, was admitted Nov. 13th, 1870. He was in good health until five days peeviously, when he was suddenly taken with pain in the left testis; it was severe, and the testes began to swell. In a day or two the right became painful also. These symptoms were succeeded by pain in the loins and head. The pain now affected him only when he stood up, or when the testes were pressed. There was very little swelling of either testis. He had some tenderness of the hypogastric region, and a good deal of straining at stool. He had no sleep at night. Temperature 103·5 deg.; pulse 80. He had thirst and anorexia. On the 15th he experienced a kind of tearing sensation at both external abdominal rings, going down into the testes. On the 16th, the temperature was 97·2 deg.; pulse 60, weak. The bowels had not been open since admission. His extremities became very cold the previous night; and he then became very hot and burning, and afterwards perspired profusely. He could not stand without support; the room and everything appeared to turn round with him. His testes were small, soft, very tender when touched, and the pain shot up along the cords. The bowels were now well opened by castor-oil, and afterwards an opium suppository of one grain was introduced. The testes ceased to be painful about four hours after the bowels acted, before he had the suppository. On the 17th, they bore handling well; he was also less giddy, could walk, but not very steadily. He had no pain anywhere. He was now taking tincture of cinchona and carbonate of iron, and by the 23rd was getting quite well. The hyperæsthesia and pain of the testes in this instance were mainly dependent on the irritation caused by fæces in the lower bowel; but the action of this cause was probably much promoted by the existence of influenza enfeebling the nervous system.

11. We should be disposed *à priori*, to expect that an organ with such extensive and powerful sympathies as the *uterus*, would be prone to suffer from sensory disorder, and such appears to be the case. The affection originally termed "irritable uterus" by Dr. Gooch is regarded by him, Churchill, Mackenzie, Tilt, and Ferguson, as essentially a neurosis. The latter expressly says "the local changes have been the fluctuating, the nervous affection the constant, element". The tenderness is so great and constant that great suffering is experienced if the patient incautiously sit down upon a hard seat, or if coition be attempted. One of the seats of this neuralgic malady is the vagina itself, which is so exquisitely tender as to render intercourse intolerable. Here I think we cannot fail to recognise hyperæsthesia as the essential morbid element—at least if we accept the *dictum* of Dr. Ferguson above cited, confirmed as it is by Churchill's authority, to the effect that in the cases seen by him there was no ground whatever for the supposition of inflammatory action. Associated with hyperæsthesia, there may be, as we have before seen, more or less of hyperæmia and inflammatory congestion; but this is not in any degree the cause of the sensitiveness. Dr. Mackenzie's statement is of practical importance, that this neurosis may be of reflex origin, and dependent on gastro-intestinal irritation. In most cases, however, it seems to be primary, and to result from some cause of nerve-exhaustion, as great fatigue, acting no doubt, on a weakly organised nervous system. The *retentissement* of this neurosis on the higher centres seems to be very pernicious; the natural emotions and faculties become miserably perverted, and all nobility of character is lost in absorbing selfishness. Sedatives, locally applied by pessary or enema. subcutaneous injections, and tonics, are our best pharmaceutical remedies; but change of scene and place, and a suitable climate, may be still more important.

12. Hyperæsthesia of the *female breast*, attended with more or less enlargement, is a well known phenomenon. It may suffice for me to have named it, and to cite Mr. Erichsen's authority for its not being unfrequently dependent on uterine irritation.

13. Hyperæsthesia *of the joints* is well known to surgeons since the publication of Sir B. Brodie's work on local hysterical affections, which I advise you all to read. The most constant symptom is morbid sensibility of the joints and adjacent parts, which is associated sometimes with more or less general tumefaction, apparently from a turgid state of the small vessels. I have known a condition of this kind, attended with sanguineous vomiting, yield to carbonate of iron, after an offer of

amputation had been (fortunately) declined. I hope none of you will ever make such a *faux pas*.

14. As regards the nerves of special sense, I cannot doubt—taking first the *optic*—that, even putting aside the case of strumous ophthalmia, where it might be contended that the photophobia was dependent on irritation of the filaments of the fifth nerve distributed to the cornea, there is really such a disorder as retinal hyperæsthesia. Mr. Soelberg Wells states that in this affection there is intense photophobia, though the eye is quite normal. There is often great ciliary neuralgia, the pain extending to the face and corresponding side of the head. One point which he mentions appears to me of especial interest—viz., that the peripheral portion of the retina is anæsthetic, while in the central vision is perfect and hypersensitive. This is very indicative of the true character of the disorder, of its affinity to paralysis. The causes are such as impair the general health or strain the retina; and the correct treatment is to rest and soothe and recreate nerve-power.

The *auditory nerve* also is liable to hyperæsthesia. Sir T. Watson mentions having observed this in a man moribund from cholera. Dr. Russell Reynolds affirms that hysteric girls do sometimes seem to hear through stone-walls. Intolerance of sound is an occasional symptom in the early period of meningitis, and is not unfrequent in nervous head-ache, and in azoturic sufferers. The only thing common to these various conditions is a lowered degree of vital power.

15. That the *cerebrum*—the great centre where impressions are consummated into perceptions—is liable to pass into a state closely analogous to that of a tertiary centre and sensory nerve when these are hyperæsthetic, seems to me not doubtful. Gooch mentions the case of a deranged lady, whose prominent belief was that her husband was unfaithful to her. The notion, so far from being unreasonable, was, he believes, true, and she had known it for many years without any unnatural disquietude, but now it engrossed all her thoughts: she neglected her ordinary pursuits, took a dislike to her friends, felt no interest about her children, and sat silent and motionless from morning to night. After continuing deranged several months she recovered, although she retained the same opinion. Gooch justly says her insanity consisted not in her ruling idea, but in its overwhelming influence over her feelings and conduct. A modern pathologist would probably express this by saying that her brain had become enfeebled and hyperæsthetic, and was, therefore, intolerant of the idea it had previously endured. The following occurrence is similar. A religious student, who was much harassed by petty scruples and fancies, and well aware of their pernicious effect, observed after a time that he

was always more plagued by them while preparing for an examination; *i.e.*, when his cerebral power was most exhausted. The essence of hypochondriasis seems to be a special hyperæsthesia to corporeal impressions emanating *ab interno*. How much the tyranny of these may depend on enfeeblement of the great centres, is shown by P. Frank's account of his own sufferings from the dread of aneurism, induced by mental exertion, and removed by recreation. This is much too large a subject for me to attempt to handle now: all I can do is to commend to your attention the notion of cerebral hyperæsthesia as essential to the due comprehension of many common phenomena.

How far the *seat* of the action giving rise to hyperæsthesia may be purely peripheral, is difficult to say. In the great majority of instances, some nervous centre is probably engaged; but some facts—such as the removal of severe neuralgia for some months after section of the nerve, and the cessation of suffering after amputation of the testis—incline me to believe that the morbid action may have its chief *locus* at the periphery, and that without any apparent structual change in the affected part.

Pathological Change in Hyperæsthesia.—You may be disposed to ask me what this is. I wish I could tell you; but our knowledge of this matter is very scanty. All that we can say is, that it is undoubtedly a condition of imperfect nutrition, inasmuch as it may be generated by deprivation of blood, by injuries to nerves, by miasms in the blood, by remote irritation—in short, by all sorts of things that are injurious. Some particulars under these heads deserve to be mentioned. Hyperæsthesia of the most intense kind is produced sometimes by obstruction of the arteries with fibrinous deposit. Dr. Fuller describes a case where the first symptom was acute pain, associated with such exquisite tenderness of the whole limb that the slightest touch caused intolerable suffering. The hyperæsthesia continued until mortification set in. At the necropsy, all the large arterial trunks, from the iliacs downwards, were found blocked up by firm light-coloured coagula of long standing. No other cause existed for the hyperæsthesia in this case, except the deprivation of blood. In Dr. Waller's experiments on refrigeration of the ulnar nerve, the ultimate effect produced was anæsthesia in the course of the nerve, but this was preceded by hyperæsthesia. It can scarcely be thought that the latter was, under the circumstances, anything but the first stage of the former; and, therefore, the conclusion seems necessary that it implies not an exaltation, but a deterioration of vital power. Again, there is a case on record where, during the gradual recovery of the brachial plexus from an injury, the

skin of the upper part of the arm possessed natural sensibility, another (probably the mid-part) was morbidly sensible, while that immediately beyond was quite insensible. Here, again, the conclusion seems inevitable that hyperæsthesia implies a damaged state of nerve-tissue, though not so severe as that which exists in anæsthesia. Much other clinical observation is to the same effect; the same morbid alteration of sensibility (cutaneous) is met with in typhus and typhoid fever, in influenza, in rickets, and in epidemic cerebro-spinal meningitis. On the other hand, there is not the least proof that real increase of functional energy renders an organ less tolerant of its natural stimuli: in fact, the evidence tends quite the other way. A vulture's eye, which can discern a carcase at an extraordinary distance, can easily endure the full blaze of tropical daylight, and is evidently altogether in a different condition from the eye of a photophobic invalid. Several similar instances might be cited; and, on the whole, the general truth of the view here taken seems to me established. In a recent lecture on Experimental Physiology, in the *Lancet*, February 11th, 1871, Dr. Rutherford has noticed this point without explaining it. He says, "the curious point, is that a nerve whose nutrition is to some extent defective should discharge its energy more readily than one whose nutrition is perfect." In what respect defective nutrition changes the nerve-tissue, I have said we cannot tell; but we may, I think, exclude œdema; for Buhl affirms as the result of his observations of continued fever, that the presence of much fluid in the subarachnoid and ventricular spaces corresponds with stupor or unconsciousness. Patients dying of delirium tremens have commonly wet brains, and they die for the most part in stupor. However, if we adopt, as I am much inclined to do, Dr. McDonnell's theory of nervous action, the detection of some material change may not perhaps be the most important matter. He assumes that the various impressions are propagated by undulations, the wave-length of which varies, so that the same conductor may be able to transmit several impressions. Now, on this view, it might be that undulations of a certain length and rapidity produced normal sensation, those of a less extent gave rise to hyperæsthesia, while a still further reduction of the undulations caused pain, and their total arrest anæsthesia. This may seem the more probable, as there is undoubtedly a close connection between the three just-mentioned morbid states. They often occur together, acknowledge the same causes, and are removed by the same remedies. Another connection of hyperæsthesia is less frequent, but is extremely well marked in rickets, viz., that with vaso-motor paralysis. I use the latter term hypothetically, for the phenomena actually observed are hyperæsthesia of the

cutaneous surface and profuse sweating. The latter, however, as I have argued elsewhere, seems to be distinctly traceable to defective contraction of vessels from a paretic state of their nerves; and if this be so, then the connection is as I have stated. Admitting this, it is difficult to see in the hyperæsthesia anything else than a species of paralysis very similar to that which affects the vaso-motor nerves, and which may itself depend on defective molecular movement.

Allied, however, as these morbid states are, there are differences between them, which are important as regards treatment; thus anæsthesia is more connected with prostration, and is more benefited by stimulants than hyperæsthesia is. The latter is sometimes intolerant of these, and yields better to remedies of the sedative class. Thus in a case of pain with anæsthesia, bark, ammonia, and wine suggest themselves; whereas if hyperæsthesia be in the ascendant, bromide of potassium or chloral, or subcutaneous opiates, seem more hopeful remedies. Yet we must not strain this rule, for it admits of numerous exceptions. Neither must we ever omit a careful inquiry into the cause of the hyperæsthesia, for the knowledge of this may be all important. It is idle to go on administering tonics or sedatives if a focus of irritation need to be removed, or if the disorder be dependent upon poisoned blood. Here is much room for the exercise of *real* diagnostic skill, not that which contents itself with affixing a name to a given case of disease.—*British Medical Journal, Sept.* 30, 1871, *p.* 369.

13.—ON THE INFLUENCE OF THE NERVOUS SYSTEM ON DISEASES OF THE ORGANS AND TISSUES, AS ILLUSTRATED BY THE ACTION OF COUNTER-IRRITANTS.

By Dr. THOMAS LAYCOCK, Professor of Medicine and of Medical Psychology and Mental Diseases, in the University of Edinburgh.

[The following is part of a lecture upon the subject of "the Influence of the Nervous System on Diseases of Organs and Tissues." After treating of the morbid results of 'trophic neuroses,' the writer asks what practical lessons in the use of remedies are to be deduced from the facts and conclusions he has brought forward. He takes the class of counter-irritants as illustrative of others; and as they are in daily use, and their *modus operandi*, after numerous recent discussions, very obscure, he endeavours to elucidate their application.]

What is meant by counter-irritation? The question is not so simple as it appears, for the solution of various and wholly distinct problems is included in it. I think, however, we may

affirm generally that the change we induce by counter-irritants locally is of the nature of inflammation, either with or without effusion, exudation, and, I may add, suppuration, in the case of setons and issues.

In all observations of this kind we must not, however, forget a general fact deduced from clinical observation and experimental research, and very manifest in the growth of cutaneous hair and scales—viz., that not only do the tissues themselves undergo normal vital changes independently of nerves or of a nervous system, but also that nerves and nerve fibrils perform their appropriate functions independently of nerve-centres. Nerves are produced anew in organised plasma, and join on to the general trunk, in accordance with the order of embryonic development of the nerves from the periphery to the centre. MM. Philipeaux and Vulpian made numerous researches, which prove that nerves separated wholly from the nerve-centres, and completely altered as to nutrition, may become regenerated, although remaining separate, and recover all their vital properties. Numerous experiments, also, on the nerves of muscles show that those motor nerve-fibrils have their own inherent properties in entire independence of brain, spinal cord, or nerve-centres, and not only in separated limbs, but in muscles that have been cut from the limbs. Unless these facts be borne in mind, it is not possible to appreciate the influence of even the sympathetic gaglia on morbid changes in organs and tissues.

Again, varying results follow, according to the nature of the irritant used. When certain drugs are the means, then, besides their local action, there is that which results from their absorption, as, for example, spirit of turpentine, which, when used as a "rubefacient," gives a peculiar odour to the urine, or that element of the cantharides-blister which excites strangury. Leaving this point out of consideration for the present, we may say further that, as to the tissues affected, all counter-irritants (amongst which heat and cold, and other physical irritants, must be included) act either locally on the tissues, including the nerves and blood vessels, or else on the nerve-centres, through the nerves, and thence, by reflex action on the same, or a distant organ or tissue. *We thus arrive at this general principle—that the laws of reflex action adapted to a trophic anatomy and to neurotic changes in tissues and organs, must be our guide in the use of counter-irritants and rubefacients.*

This being so, one practical conclusion follows directly—viz., that for the purpose of effective counter-irritation, beyond merely local results, it is necessary that there be continuity of sensory or afferent trophic nerve between the surface irritated and the nerve-centre to be acted on, otherwise no change can be

effected therein. Again, if there be no continuity of efferent or motor nerve between the part to be modified and the nerve-centre which modifies, no counter-irritation will avail, although the sensory communication be continuous. Thirdly, since any surface may be made available for exciting reflex action, so in like manner other surfaces than the surface of the body may be available for counter-irritation. Hence, when we excite vomiting by tartar emetic, purging by croton oil, scammony, or other drastic purgatives, and strangury by cantharides, we counter-irritate as certainly as when we apply sinapisms to the feet, fumes of ammonia to the nostrils, and fly-blisters, turpentine, or chloroform to the skin. Fourthly, we may go further, and say that certain inflammations of these surfaces, induced otherwise than by counter-irritants, will have a diastaltic action analogous to that of counter-irritation.

Having thus, then, widened our view of matters, let us take up a few practical points. Firstly, we desire to alter the condition of the nerve-centres in centric diseases by counter-irritants. The time-honoured use of sinapisms to the feet and wrists in apoplexy and comatose affections generally, is a familiar example of direct irritation through afferent nerves; so, also, the irritation of the nasal branches of the fifth nerve in cases of failure of the heart's action; but you may widen much the sphere of these remedies. For example, I have seen cephalic snuffs of great use in epilepsy, and I have no doubt they might be applied with advantage in other encephalic diseases of defective nutrient and vascular activity. The ancient Greeks used this class of remedies so systematically in head affections, that they invented a double-piped syringe for the purpose of injecting counter-irritants into both nostrils at once. In cases in which it is advisable to stimulate the nerve-centres through the afferent nerves, hot applications, as Donovan's button, may be applied, or rubefacients, which excite more or less pain and tingling. In certain cases of paraplegia I have found it very useful to cover the lower extremities with sinapisms for half an hour two or three times a day, so as thereby to excite vaso-motor activity in the motor cord through the sensory nerves. In these cases it is necessary that there be at least sufficient integrity of the sensory nerves and nerve-centres to evolve and transmit the regulative or sensory vis nervosa. If there be complete anæsthesia from structural change, little good will be done; but if the lost or diminished sensibility be only functional, you may re-excite centric sensory activity by stimulating the sensory nerve-fibrils, in accordance with the Wallerian law of line of physiological activity that I previously explained to you; for, just as sensory centric disease may begin at the periphery, so, also, may the

cure. This rule applies to all those trophesies in which there is sensory defect, but not to the extent of absolute anæsthesia—a mere trophic debility being the result. In this numerous class there is commonly a defective evolution of both motor and sensory vis nervosa; but as the evolution of the one is excited by the other, it is plain we must act therapeutically upon the sensory system in the first instance, and this can be done by counter-irritants. It will be a daily problem for you to determine how far pain or tenderness on pressure should indicate, or contra-indicate, the use of counter-irritants. The facts to ascertain are the causes of the pain. Is it due to local causes, as when a wound is inflicted, or, if there be local causes, has pain preceded or followed them? It is too often assumed that the pain is wholly due to local changes; whereas it either precedes or coincides, as in certain sensory trophesies. The connection between pain, as neuralgia. and metastasis in rheumatism and gout, shows how certain trophic changes are dependent on the sensory system. A morbid change is often the more dangerous, in fact, because painless, as in the case of paraplegia I quoted, or because it ceases with cessation of pain, as in metastasis. In examples of this kind, pain is only a stage to that trophic palsy which is indicated by complete anæsthesia. Hence the importance of studying morbid-tissue change in relation to pain and painful states, whether of body or mind, and under this head the relations of neurotic diseases of organs and tissues to mental work, and to mental pleasure and pain.

It may be held as a general principle that diminution and loss of sensibility are associated with defect in that centric regulative property by which tissues and organs resist causes of disease. Now, rubefacients and counter-irritants are such causes; so that, when applied to the skin, when there is that defect I have termed trophic nervous debility, the results may be a ready morbid reaction to the counter-irritant far beyond that desired. This kind of ready reaction is seen in the skin in cases in which there is lowered innervation—as in measles and other exanthemata, in low forms of typhus, diphtheria, and influenza. In such specific fevers fly-blisters for pneumonia are worse than useless, being likely to bring on low inflammation, if not sloughing. There is a like tendency in this class of diseases to low inflammatory action of the skin from pressure, and of the cornea from slight irritants, just as is seen in paraplegia and in the eye and nostrils in disease of the Gasserian ganglion. Such facts alone would indicate that in specific fevers the trophic nervous system is involved.

On the other hand, in a numerous class of cases—in those, for example, termed "idiopathic inflammations"—there is a

healthy or normal reaction against injurious agencies, and, of course, a comparatively healthy nervous system. How shall we use counter-irritants in these? I think we can still fall back upon the laws of reflex action for guidance. When an irritant is applied to a nerve, and excites reflex movements, they are of a conservative character; so, also, when it excites the reflex nutrition, which is the result of its action on the trophic system. But this is only seen when the regulative or sensory system retains its functions; otherwise there is no healthy reaction, but rather a tendency to what is unhealthy. If, then, the counter-irritant fails to excite the afferent and regulative system normally, it is useless, and may be injurious. Pain is a natural sign of injury. When, therefore, there is tenderness on pressure, or pain referred to the regions of an inflamed organ, would that indicate the use of counter-irritants? Here, again, the question is more complex than it appears on first consideration. In some cases there is hyperæsthesia of the skin only, and no pain in the organ than what may arise from over-distension or other local and merely mechanical causes. Further, the pain is not always referred to the seat of the inflammation, but elsewhere. Thus, occasionally, in both pleurisy and pneumonia, it is not experienced in the affected side, but in the opposite; so that the question arises whether decussating nerves are affected sensorially, and direct trophic nerves motorially. Upon the whole, in acute cases, both experience and theory are opposed to violent (*i. e.*, inflammatory) counter-irritation—rubefacients, at the most, are all that are needed; but even these are more doubtful than local sedatives with warmth and moisture to the skin. The local morbid condition has already caused those centric changes upon which pain depends, and the normal reaction has followed; this being so, it seems advisable to relieve the pain rather than increase it. In this way the great additional suffering may be spared the patient, which the "heroic" use of blisters, tartar emetic, croton oil, and other inflammatory counter-irritants inflicts.

I think we can also make these views available to a better comprehension of the uses of counter-irritants when applied to induce absorption of fluid in dropsical joints and elsewhere. Do they act directly and solely on the absorbents, as is generally believed, or on the nerve-centres as well? When a blister is applied to the side to excite absorption, it is more difficult to understand how it acts locally than through the nervous system, if we remember that the so-called serous metastasis is nothing more than the rapid absorption of serum from a change in a nerve-centre, as in the case of dropsy I alluded to, in which rapid absorption followed on embolism of the

middle cerebral artery of the same side. Again, if we give the so-called hydragogue cathartics in acute hydrocephalus, ascites, and general dropsy, we use very active counter-irritation of the intestinal surfaces, and in this way act as certainly on the nerve-centres as when blisters and rubefacients are applied to the skin. So, also, when we use the hot-air bath in dropsies, we do much more than stimulate locally; we apply a powerful agent to the nervous system. On the other hand, blisters applied to joints under certain conditions will excite effusion.

I could multiply illustrations of this kind, if necessary. Irritants to the stomach, for example, act as counter-irritants on the lungs as well as on the brain; irritants to the kidneys, urethra, and cervix uteri, are counter-irritants to both brain and spinal cord—in short, all the diastaltic actions of irritated mucous surfaces and of morbid viscera are of the nature of counter-irritation, and may be induced artificially. I will only mention one curious illustration:—Irritation of the urethra not unfrequently excites violent rigors. The New Zealanders adopt a coarse means for this purpose, as a counter-irritation for the cure of tetanus.

A clinical trophic anatomy should enable us to observe and treat two kinds of morbid neurotic changes—viz., those which occur in the nerves and nerve-centres themselves, and those which occur in organs and tissues in consequence of those changes. We have only studied the latter class; we will now study the former.

Let me premise as a fundamental fact that, apart from certain morbid states of the blood and of the auxiliary tissues, or what have been termed coarse and mechanical diseases of the brain and nervous system, every change in a nerve or nerve-centre is, for the most part, the result of a change in some other portion of the nervous system. So that, to understand the true causes of numerous neuroses, it is necessary to trace the order of events which lead up to them. This we have done generally, and without special reference to the kind of morbid changes which take place; now we have to specialise them as far as is practicable and practical.

In my lecture on the Clinical Observation of Diseases of the Brain and Nervous System, I explained to you the general rules to be observed so as best to ascertain the various changes therein which constitute the predisposing, exciting, and proximate causes of neuroses in general. When, however, we have to consider those of special nerve-centres, it is necessary to determine, at least, two things—viz., the seat of trophic changes in the nerve-cells and nerve-fibrils, and the relations of vaso-motor action to these changes. As to the nature of

the trophic and dynamic changes, I need only remind you of this—that no anatomical research, however minute and microscopic, whether conducted in living or in dead tissue, can reveal to us those molecular changes upon which the varying functions of the nerve-centres depend. All we can say positively of these, as of molecular chemical changes, is, that there are such changes, and that each kind must occur in its own proper portion of the nervous system, in accordance with the functions of that portion, and according to fixed laws. All visible structural changes in nerve-tissue are coarse diseases, and coincide with destruction or abolition of function. They indicate the results of trophic disorder, often of long standing, but not the causes of the disorder itself.

In respect to the vaso-motor activity of these nerve-centres we know more—but little that is positive and practical. We have not determined the vaso-motor relations of the nerve-centres to each other; anatomical and physiological research have failed as to the most fundamental centres—the sympathetic ganglia; so that our ignorance of their functions is generally admitted. When we turn more especially to those ganglia which regulate the activity of the blood-vessels that supply the encephalon—viz., the cervical ganglia—we have not only to deal with the carotid and vertebral systems, but with that of the thyroid body and its relations to the functions of the brain and spinal cord. Looking at these relations from the anatomical and vaso-motor points of view, we cannot avoid the conclusion that the thyroid body has more complex trophic functions than is generally suspected. This is shown by its twofold blood-and-nerve supply. The *nervi molles* (so called) are the branches of the system of the superior cervical ganglion which accompany the arteries of the carotid system; these also accompany the superior thyroideal artery as a limb of that system, whereas the inferior thyroideal artery belongs to the system of the vertebral arteries, and receives nerve-fibrils from its own ganglion, the middle cervical. We may reasonably conclude, therefore, that as the innervation and arterial supply of the thyroid body are double, the functions are double. That these functions are in connection with those of the other cervical ganglia is probable from the intricate anatomical relations of the whole chain; these are so close that the thyroideal ganglion is sometimes integrated (an important fact) with the lower cervical, while it has commissural connexions, not only with the superior cervical above, but with the cardiac ganglion below. Now, no one, so far as I know, except myself, has called attention to the clinical relations of these facts to goître and cretinism, to vascular bronchocele and "Graves' disease," and to the functions of the ovaria and uterus.

How much considerations of this kind should make us hesitate in drawing conclusions from experimental researches into the functions of the cervical sympathetic is plain enough; and it is precisely such considerations which enable us to understand why, with all the laborious research I have alluded to, it is a general complaint that we know so little, practically, of the pathology of the sympathetic system. According to vaso-motor theories, the cervical sympathetic ganglia should regulate the encephalic circulation; consequently, in encephalic neuroses like epilepsy and its serious accompaniments of morbid brain-nutrition, leading to insanity and dementia in fevers with delirium, and in insanity in general, the pathological anatomy of these centres should elucidate the pathology; yet how little of this is known! They have been found red and swollen in a few cases of death from hydrophobia, and from typhus; but of their state in insanity, epilepsy, hysteria, and various vaso-motor affections of its encephalic tissues, the circulation within which is supposed to be regulated by these ganglia, we have few, if any, observations. The cause of this neglect is not far to seek: it is the chaotic conflict of views as to the anatomy and physiology of the sympathetic system. Feeling strongly these difficulties, I have lately adopted a new method of observation of this great group of encaphalic neuroses, which certainly helps to simplify bedside work and to clear up numerous doubtful and embarrassing problems. I shall, therefore, explain it to you. To this end I will first, however, explain the guiding principles of the method.

I have already on various occasions indicated the clinical and practical uses of those great laws of evolution and development, which are termed transcendental, and which hitherto have had little attention from Practitioners and teachers. I once again turn to this department of biological science for help. It is generally yet erroneously thought, in accordance with scientific terms, that the nerves "arise" and are developed from the nerve-centres, and the arteries from the heart and large vessels. Hence the numerous discussions as to the "origin" of the sympathetic, and of the "roots" of various nerves both spinal and encephalic. Now, in the development of the embryo the nerves are formed independently of the nerve-centres, and the arteries appear before and act independently of the heart; so that the evolution of the vascular system, with its accompanying nerves, is not like that of the branches of a tree from a common trunk, but like that of a river from a number of rills, or like that of veins. The function of the central ganglia is to unify trophic changes in tissues and the actions of the vessels and of their accompanying nerves; but these changes and actions can and do go on independently of

either heart or nerve-centres. If we go a step further and inquire to what uses vessels and nerves are subservient, we learn at once that they subserve to cell-function, and that cells can be grouped together, each group having distinct functions to perform in the body politic. My late friend and colleague, Professor Goodsir, first promulgated the doctrine—afterwards confirmed by Virchow—that not only is the entire organism composed of simple or developed cells, each having an independent vitality, but that there is also a grouping of cells into departments around one capital or central cell. A common and striking example of this kind is the primordial cell of the ovum. I have pointed out in my psychological text-book how the laws of the fundamental process which go on in these cell-groups may be formulated for practical use as the correlative laws of life and thought.

Guided by these facts, I take an area of blood-supply as indicative of an area of cells and tissues in functional and trophic relation with each other, and with a common source of blood and of regulative vis nervosa, both vaso-motor and trophic. These areas may be marked out in the encephalic tissue, and as special centres of functional cell-activity, in two ways—viz., by the descriptive and pathological anatomy of the arteries, and by the observed functional and trophic changes in the corresponding areas of blood supply; only, instead of looking at the arterial trunks as they give off branches, we must consider them as made up of branches. I think Serres was the first to show, in his "Anatomie Compareé du Cerveau," published in 1824, that the development of the encephalic nerve-centres is dependent on the development of the arteries, and that the nerves, whether sympathetic, spinal, or encephalic, grow as it were towards the nerve-centres with which they are finally connected. Since, as the areas become more and more comprehensive, a larger blood-supply is needed and more regulative vis nervosa required, it follows that with an increased extent of central control the arteries get larger and the nerves thicker; so that each important trunk or branch of nerve and of artery may be held to indicate a centric trophic area. Guided, therefore, by this principle, I have divided the cerebro-spinal axis into distinct yet mutually dependent arterial areas, each presumably with its sympathetic ganglia, its commissural connexions, its correlative cranial and other structural developments, and its sphere of physological and pathological changes in organs and tissues. The method is so wholly new that it is as yet imperfectly developed; you will, however, find these arterial cerebro-spinal areas set forth in a tentative way in my psychological text-book. On future occasions I shall apply them to the study and treatment of hysteria,

epilepsy, and insanity; at present we will see how far they will help us to a knowledge of the relations of the encephalic vaso-motor or trophic centres to neurotic diseases of tissues, both within and without the cranium.

Serres, in the work I referred to, lays down two laws of development of the central nervous system—viz., the law of symmetry or of symmetrical halves, and the law of conjugation or of integration of the two halves of the nervous system into one organ. The cerebellum, for example, is fundamentally double—a lateral leaflet coming from each restiform body, representing in fishes that which in man constitutes the hemispheres. The median lobe (or superior vermiform process) is developed in relation with the tubercula quadrigemina. The arterial development follows the same laws. On each side there are symmetrical arteries, and where there is conjugation of nerve-centres we find conjugation of arteries either by integration or by anastomosis of the trunks. When the centres are distinctly ganglionic, united by commissural nerve-fibrils, we shall have arterial trunks united by anastomosing arteries; but where the two symmetrical halves combine, both by commissure and ganglion, there may be union of the arterial trunks of each half, either by complete integration into one trunk or by a cross anastomosing branch; so that anastomosis and integration of arteries, and *vice versa*, represent commissural union of nerve-centres in relation to them. The aorta is thus constituted out of two lateral vessels. It has, therefore, a corresponding ganglionic and commissural centre somewhere in the cord or the encephalon. The integration of the two vertebral arteries into the basilar artery at the lower border of the pons Varolii, and in exact accordance with its length, indicates that the pons is the commissural centre of those groups of cells to which the blood is distributed from thence. This great vascular area is in functional relation, therefore, with the most complete commissural centre of the encephalon and its dependencies—the "vital knot," as the French anatomists, with much propriety, term it. Nor is this coincident unity of arteries and development of correlative trophic centres a solitary fact, as we shall soon see, but a general law. After the union of the two vertebrals with the basilar artery, two subordinate areas of innervation in connexion with the pons are marked out by two distinct sets of branches from the basilar. One of these includes the cerebellar arteries, and supplies the cerebellum, pons Varolii, medulla oblongata, and the upper portion of the spinal cord. This cerebellar vascular area is further divisible into two areas, supplied by the posterior and anterior cerebellar arteries respectively. Of these, the posterior as that which corresponds with the commissural connexions of the cerebellum

with the cord; for, although often found to be given off from the basilar, it is not unfrequently a branch of the vertebral artery on one side, and of the basilar on the other. These two cerebellar vascular areas indicate, therefore, a group of different nerve-centres, which may be referred to the two commissural vermiform processes; for while the posterior or cerebello-spinal artery is ultimately distributed to—which means in development that it primarily arises in—the inferior vermiform process and the sides of the median fissure, and the inferior surface of the cerebellum, the superior cerebellar artery supplies the superior vermiform process (median lobe), the velum interpositum, and the valve of Vieussens. A small branch arises in the internal auditory meatus, and this seems to correspond to that branch of the auditory nerve which has been traced to the cerebellum. Tinnitus aurium and spectral voices thus acquire a diagnostic significance as to the state of the circulation in cases of brain disease of this occipito-spinal region. A few branches go to—*i. e.*, in development, come from—the under surface of the sphenoidal lobe, thus connecting this cerebellar branch with the function of that lobe.

To understand clearly, however, the important clinical relations of these areas to epilepsy and insanity commencing therein, we must include the blood-supply of the basilar region of the cerebrum and of the ganglionic centres situate therein. We know little of the functions of several of these; but so much is certain, that they appear early in the scale of development, and are obviously in essential connection with the corporeal centres proper. The two posterior cerebral arteries of the basilar area indicate the bond of union. They supply blood to the inferior surface of the posterior lobe, to the crura cerebri, tuber cinereum, and corpora albicantia, while a choroid branch connects the velum interpositum and tubercula quadrigemina with the basilar area, as well as with the vascular choroid plexus. All these encephalic nerve-centres and tissues are therefore within this great occipito-spinal and corporeal area of vaso-motor activity, to the neuroses of which the vaso-motor activity of the vertebral arteries is the clue. Their vaso-motor connection with the vascular areas supplied by the internal carotid system is indicated by the posterior communicating arteries. According to my view, these correlate a corresponding commissure, and this, I think, is the fornix or great inferior longitudinal commissure of the hemisphere. So that the posterior communicating arteries are to the fornix of each hemisphere what the basilar artery is to the pons Varolii.

Looking, then, at the anatomy, physiology, and pathology of the encephalon from this new point of view, we can broadly allocate function, blood-supply, and trophic change

to three distinct regions, any one of which may be involved morbidly without the other manifesting change, yet connected with each other, and of which each symmetrical half may be involved without affecting the other half; these are—(1) the corporeal region, including the medulla oblongata supplied by the cerebellar arteries; (2) the animal, supplied by the posterior cerebral; and (3) the mental proper, or intellectual, supplied by the internal carotid arteries. The commissural union, or conjugation of this last-mentioned region, is indicated by the anterior commissure; and its correlative anterior development is either the anastomosing anterior communicating artery, or, as is sometimes found, integration of the two anterior communicating arteries, so as to form one trunk, like the basilar. Thus the circle of Willis is constituted so as to represent in the arterial system the commissural connexions of the encephalon.

Various difficulties and objections at once arise at first consideration of these facts and conclusions. Primarily, it will be said that these anastomosing branches are simply for the purpose, and no other, of equalising the circulation within the brain, and providing against any cutting off of the blood-supply to any particular part; this is what the anatomical books teach. Now, seeing that one half of the brain may do the work of both halves, just as one kidney may do the work of both, it is important that there shall be a ready diversion of the proper supply of blood to the working half; and to that extent the explanation is satisfactory. But we have to consider more than the working of a hydraulic machine when we turn to the changes which occur in the blood, in the capillaries, and in the cells and tissues to which the blood is distributed during functional activity of a portion of brain-tissue; so that, although the equalisation of the blood-supply be attained, we have still to explan the vaso-motor and trophic changes in the respective nerve-centres in relation to each other, which my method helps.—*Medical Times and Gazette, May* 27, *and August* 11, 1871, *pp*. 595, 211.

14.—CALABAR BEAN AS A REMEDY IN CHOREA.

By Dr. W. H. Fuller, Physician to St. George's Hospital.

[In consequence of a paper by Dr. John W. Ogle (*Retrospect*, vol. lii., p. 71); and another by Dr. Maclaurin, of Greenwich, reporting cases of chorea successfully treated with calabar bean, Dr. Fuller determined to give the remedy a trial.]

As these were the first, and, as far as I am aware, the only occasions on which the drug had then been exhibited as a

remedy for chorea, it occurred to me that before accepting the favourable conclusions to which those three cases pointed, it would be advisable to follow up the experiment so begun, with the view of ascertaining whether the drug was indeed what it appeared to be, namely, a valuable remedy in this troublesome disorder. The results of my experience, I regret to say, have not confirmed the newly acquired reputation of the drug. I gave the bean in the form of tincture to seven consecutive cases, which were all of average severity. The drug was administered alone, the dose at the commencement being ℳx. of the tincture* mixed with an ounce and a half of water. This was usually given three times, but in two instances four times a day, and the dose was increased gradually but rapidly, usually by about ℳv. every second day, until a point was reached at which the characteristic poisonous effects of the bean began to manifest themselves. The pulse became accelerated, feverishness supervened, loss of appetite and vomiting ensued, and uneasy sensations were complained of in the head. A drachm and a half or two drachms of the tincture taken three times a day usually sufficed to induce these symptoms. Beyond this, however, no appreciable effect was observed. There was no evidence of the cumulative action of the drug; the drug did not appear to influence the secretions; it did not materially affect the pupils; and it certainly did not exercise the slightest influence over the disease. Thus I am forced to the conclusion, that whatever other claims the physostigma may have to be regarded as a valuable internal remedy, it has none as a curative agent in chorea; and that the improvement which was observed to follow its administration in the two or three cases previously recorded, resulted probably from the careful regulation of the diet and the general improvement of the health which accompanied the patients' residence in hospital.—*St. George's Hospital Reports*, *Vol.* v., *p.* 13.

15.—TURPENTINE IN NERVOUS HEADACHE.

By Dr. J. Warburton Begbie, Fellow of the Royal College of Physicians, Edinburgh.

[The following is from a paper on the therapeutic uses and actions of turpentine.]

In this painful affection, occurring in young persons of a delicate excitable temperament, without any menstrual or leucorrhœal complication, Dr. Graves placed great reliance on

* Made by macerating one drachm of the powdered bean in one ounce of rectified spirit.

turpentine. He gave it in doses of one or two drachms, to be repeated according to its effects. The best vehicle, he adds, is cold water. Some will bear and derive advantage from two or three doses of this medicine in the day, experiencing from its use a diminution of headache, and removal of flatulency, together with a moderate action of the bowels and kidneys. There is, moreover, another class of sufferers from headache, and this composed of both sexes, who may be relieved by turpentine. I refer to the frontal headache which is most apt to occur after prolonged mental effort, but may likewise be induced by unduly sustained physical exertion, what may be styled the headache of a fatigued brain. A cup of very strong tea often relieves this form of headache, but this remedy, with not a few, is perilous, for, bringing relief to pain, it may produce general restlessness, and, worst of all, banish sleep. Turpentine, in doses of twenty or thirty minims, given at intervals of an hour or two, will not only remove the headache, but produce, in a wonderful manner, that soothing influence to which reference has already been made. One dose is not uncommonly sufficient, but it is rarely necessary to repeat the remedy for more than two or three.—*Edinburgh Medical Journal, July*, 1871, *p*. 50.

16.—A NEW TEST FOR HYSTERIA.

A French work just issued by Dr. Chairon, Chief Medical Officer to the Vesinet Asylum, entitled "Clinical Studies on Hysteria," announces the discovery by him of a new pathognomonic sign of hysteria, which, should it be confirmed by experience, will prove to be a valuable contribution to medicine. Since Dr. Chairon has become connected with the Institution, he has passed under view 26,000 female patients, amongst whom were a great many cases of hysteria. He says that he has ascertained that in every one of them the commencement of the affection has been marked by a special sign—insensibility of the epiglottis.

The determination of this symptom, which is constantly present, is very simple. It is sufficient to introduce gently the finger into the mouth, so as not to frighten the patient, and place it on the base of the tongue. It will be found that the epiglottis may be touched, displaced and scratched with the nail without producing the least regurgitation. When this symptom exists there will be found invariably a congestion of one or both ovaries, usually of the left.

Singular as this proposition is, the author proceeds to prove its exactitude, and has, with that object, quoted a great number of cases collected at Vesinet.—*Medical Press and Circular, May* 3, 1871, *p*. 381.

DISEASES OF THE ORGANS OF CIRCULATION.

17.—ON THE ACTION OF THE HUMAN HEART.

By the Rev. SAMUEL HAUGHTON, M.D., D.C.L., F.R.S., &c.

[The story of the heart is a wonderful and mysterious one, and deeply interesting both to us personally and as medical men. The following is from one of Prof. Haughton's lectures at the Royal Institution, upon the principle of Least Action in Nature, —in other words, the principle that nature will effect a certain result with the least effort possible. Some of the results obtained and explained in this paper have been the product of very great labour.]

We have first to consider the question of *the amount of work done by our hearts*. The heart is a small muscle, weighing only a few ounces, beating perpetually day and night, morning and evening, summer and winter; and yet often an old man's heart nearly 100 years of age is as perfect and complete as when he was a young man of 20. In order to measure the force and power of the human heart, the most obvious way is impossible, because it would require the death of the person on whose heart the experiment was made. We have experimented on the hearts of horses, oxen, dogs, and sheep, however. The first of these series of experiments were made by the celebrated Dr. Hales of the last century, who measured, by direct experiment on the vessels of these animals, the amount of hydrostatic pressure to which they were subjected. These experiments showed that it varies in the horse and ox, amounting to about nine feet; and in the smaller animals to somewhat less. We can calculate the total amount of work done by the heart of an ox, by the heart of a horse, by the heart of a dog, or of a sheep. But it would be impossible to perform such an experiment upon man, because the experiment is accompanied with certain death.

The co-efficient of capillary resistance must be determined. The heart pumps the blood into the capillary vessels, which permeate every tissue of our frames, and the greatest resistance lies in these capillary vessels. The co-efficient of capillary resistance, as I have determined it in some of these animals, is as follows:—in the sheep, $\frac{1}{186}$; the dog, $\frac{1}{196}$; the horse, $\frac{1}{393}$: the ox, $\frac{1}{398}$.

You observe that in the sheep and dog the co-efficients are double those of the horse and ox. These large and small animals naturally group themselves together. Now, with which of these groups of animals are we to associate ourselves in making a calculation of the amount of work done by our hearts? We cannot perform the direct experiment, but an

accident on one occasion placed in my power the means of making a close approximation to the true result. When the artery of the horse or cow is cut we can measure with ease the height to which the blood will spout. But we find that the artery does not spout to the height of nine feet, as we should have supposed. There is a hydrostatic pressure of nine feet in the horse, but the blood will only spout to the height of two feet and a half. The reason of this is, that Nature shuts off the pressure to prevent the loss of blood. If we could by any process arrive at the knowledge of the height of blood from the wounded artery of man, and compare it with that of the horse, cow, and sheep, we should find the animals to which man is most closely allied in this respect are the larger. On March 18, 1863, in the Meath Hospital, Dublin, an operation was performed on a poor man in whom I was interested. I was merely a spectator, and was therefore able carefully to watch the following phenomenon in the course of the operation. A large artery was cut through, because it occurred in an unusual place. The blood spouted in jets for a minute or for two minutes before the artery could be tied. When the operation was over, I examined with care the height of the table on which the man lay, and the exact position of the furthest spurts of blood upon the floor; and by the application of a little geometry and mathematics to this problem, I was able to ascertain—by taking the spots of blood thrown to the greatest distance—the velocity with which the blood flowed from the wounded artery. The course of the blood is a parobola, and, two points being given, we can construct the angle of elevation and the velocity of the fluid. As soon as I had made this calculation, I found that, if I cut the artery of a man, it would spout to the height of 2·58 feet, or rather more than thirty inches. Now, taking the mean of all Dr. Hales's experiments upon horses, I find that it is 2·53 feet. We cannot, indeed, compare the hydrostatic pressure of the human heart directly, but we can by this determination of the velocity of the spouting blood show the force of the circulation in our system, and apply to it the co-efficient of resistance which we find directly from the horse and ox. When this co-efficient of the hydrostatic pressure inside the human heart is used, and knowing how often the human heart beats—seventy-five times every minute—we can calculate the amount of work done in a given time by the human heart. This work I shall represent in an extremely easy way, and show you the extraordinary amount which it attains. If I cut out an ounce of muscle from the heart, and ask myself the question—What number of pounds can that contracting muscle lift in the course of a minute? I find that a single ounce of the human contracting muscle will lift 20·576 pounds

through the height of a foot in a minute! This I believe to be a close approximation to the power of the heart; but, inasmuch as it is not obtained by direct but by indirect reasoning, I tried to verify it in another way; and in verifying it by a second process I made use of an interesting phenomenon originally observed by the celebrated Dr. Wollaston, and recorded in the *Philosophical Transactions* for 1809. He was the first person who noticed that *when our muscles were contracting they gave out a deep note.* If any of you wish, you may try it for yourselves. If you go into a room alone, and, in perfect silence, place your elbows on a table and place your fore-fingers lightly in the ears, clenching the hands at the same time, you will hear a sound you never heard before—a deep, low hum; or, if you waken at night and clench your teeth, so as to call the masseter muscles into action, you will hear a hum, the pillow acting as a sounding-board. You can, therefore, soon satisfy yourselves that the action of a muscle is accompanied by something which produces a musical note. My attention was directed to this in a remarkable way. A young Physician of Marseilles—Dr. Collongues—was directed to the study of this curious phenomenon in examining some of the cases of cholera which died. I found the same fact in Dublin at the same time, and quite unknown to Dr. Collongues, as his discovery was to me. The patient in cholera has his temperature much lower than in health. 98° F. is the natural heat of the blood, and it is well known to Physicians that if it rises some 7° or 8° the patient will die; and so, if the temperature of the blood falls about the same number of degrees, the patient will die. But it is a strange fact that, if you examine the body of a patient who dies of cholera, it is warm. The temperature rises after death to 103° or 104°, as if the person was in a fever. This is accompanied by the movements of the limbs which cause such great alarm to those who know no better. Happening to place my ear against a person who had just died, I heard a hum. I placed my ear to the heart, to see if he was yet alive, and if I could save his life, but he was dead. His muscles still continued to live after the heart, which has been called *primum viveus ultimum moriens*—the first to live, and the last to die. After the heart has ceased to live and the brain to think—after the man is dead—the muscles still live and have the last traces of life in the body, like the last notes of the harp which vibrate when the master's hand has ceased to play. I constructed a number of organ-pipes, and succeeded in imprisoning the muscular hum in one of them, where I could afterwards measure its correct kind and force at my leisure. The muscular hum which vibrates is C on the musical scale, two octaves below the C of the bass staff, or D,

two octaves below D on the bass staff. I found that it is certainly between C or D, and by fixing a second organ-pipe to my own muscles, and comparing them, I was able to ascertain with a considerable degree of precision the exact hum. This I made to be $35\frac{1}{3}$ vibrations per second.

Well, after I published this result, Collongues, who had removed to Paris, sent me a book, in which he had succeeded in proving by tuning-forks that the number of the vibrations of muscular contraction is thirty-six. This very remarkable result, obtained by two persons with different intruments, attracted attention, and Dr. Collongues was uneasy about the priority of the discovery. I went over to see him. When I admitted his priority, and said that I was more pleased to find that we had both found out such a remarkable fact than that I should have been the first in its discovery, he embraced me. My object in determining the musical note was to calculate by a second process a co-efficient to represent the work an ounce of muscle could perform. I also placed my arms horizontally, and held them out in that position until they were completely tired, placing weights upon them to produce this result. In this way I tried to find the force with which the muscular contraction took place by means of the musical note. These vibrations seemed to occur thirty-six times in a second. A second co-efficient of power gained from the above experiments came out to be 20 lbs, to be lifted by an ounce weight of the heart every single minute through one foot. This comes in an extraordinary degree close to the hydrostatic amount. I am therefore entitled to consider that 20 lbs. can be lifted through a foot by every ounce of my heart in a single minute. But this conveys to you no adequate conception of the work done by the heart. I therefore devised a plan of comparison for showing you how much you ought to wonder at the great work of the heart. I obtained from Mr. Main, of Oxford, and Mr. Maclaren, the trainer, the cross-sections of the Oxford eight, and other particulars. The time in which this race has been done is on an average 23 minutes $3\frac{1}{2}$ seconds, and the length of the course 4·31 miles. From these data, and from plans and sections of the boats, I was able to determine the amount of work done by the muscles of these young men. I find that, during the twenty-three minutes that the race lasts, every ounce of muscle in the arms and legs is working at the rate of 20·124 lbs. This comes out very like the work of my heart at this present moment. I am not sure but that my heart is just now doing more. If any of you have seen the exhausted condition of these young men, when lifted out of the boats, you will agree with me that human beings could not endure such exertion for forty minutes; and yet an old man, 100 years of age, has a heart which has worked

all these years as hard as the young men of the Oxford and Cambridge races!

We have now discussed—and, I trust, to your satisfaction, solved—the question how much work is done by the heart. But the question is naturally asked—How does the heart do that work? I cannot pretend to tell you how that is done by nervous supply, as that is beyond our present knowledge; but I have succeeded in getting some information from the muscular fibres of the heart. I have applied a strict and rigorous testing of least action to the structure of the heart, so as to ascertain, if possible, some law which must be fulfilled by the arrangement of the fibres allowing this principle to be carried out. The law of muscular contraction which must be complied with is this:—Let the length of the muscular fibre be fixed; the order comes from the brain, or some other nervous centre, for this fibre to contract; *it must be shortened to eight-ninths of its length.* Now, wherever a group of fibres are so arranged, as in the triangular muscle, we saw that each fibre could not contract to this extent. So we must find some principle in the heart by which every fibre can contract to eight-ninths of its length. Borelli compared the arrangement of the heart's fibres to a ball of twine; but it is more like two balls of twine wound up inside a third. The whole heart consists of two ventricles, certain groups of fibre running round one cavity, and certain groups running round another. These are called the proper fibres of the heart. But there is a third group of fibres incasing the whole, and these are called to common fibres, because common to both cavities. I have taken my diagrams from Dr. Sibson's book on this subject. These fibres start from the tendinous zone, wind round the heart to the apex, and then run back to the zone internally. The outer fibres enter the heart at the apex, and in their return form the lining and internal parts of the heart's cavities. Imagine millions of these fibres, and you will have an idea of the complexity of the form of the heart. We have three systems of fibres running round these cavities, then, according to extremely beautiful geometrical law. The law is—that *the spiral fibre which goes round the entire two cavities of the heart describes* 180 *degrees before it returns; whereas the others describe an entire circumference and a fifth.* This fifth is to give a twist to the cavity, to prevent the blood, even the least quantity, remaining in the cavities, just as you would give an extra wring to a cloth. I have to thank Dr. Sibson for placing at my disposal his unrivalled preparations of dissected hearts—indeed, he placed all his stores of knowledge at my disposal. As he is not present, and therefore cannot blush at what I say, I may add that his great knowledge of the heart is fully equalled by the kindness with which he places his knowledge

at the disposal of the humblest student. Jealousy is so generally a quality of scientific men that it is a great pleasure to find one without it. But I suppose this quality of jealousy in scientific men may be regarded as raising them to the level of the gentler sex!

Each of these fibres is so arranged, then, that it contracts to eight-ninths of its length, and the lengths of each group of fibres are the same. Since this is so, you will easily see that, as far as they are concerned, the principle of least action is fulfilled. But there is a manner of applying a crucial test to the principle of least action here. I have got two groups of fibres —one surrounding two cavities, and another surrounding one cavity only. By the application of geometry I was able to prove a remarkable result. If I call L the length of one of these spiral fibres, the volume of the whole will be proportional to the cube of L—so that if I had two hearts I might calculate the difference of their volume. The difference of the heart before and after contraction is the sum of the volumes of the two cavities, thus—

$$\frac{\lambda + \rho}{\lambda} = \frac{L^3 - L'^3}{l^3 - l'^3}$$

And so, also—

$$\frac{L^3}{l^3} = \frac{\lambda + \rho}{\lambda}$$

where L is the length of the common fibres, l is the length of the proper fibres, and λ and ρ are the volumes of the left and right ventricles.

There are theoretical grounds to lead us to suppose that these two cavities are of equal dimensions. But I have taken the mean of measurements made by ten observers, and find that the mean is 2·125 inches. From theoretical grounds, I believe that more accurate observations will lead to 2. I measured the outer fibres of many hearts of oxen, and found them 10·875 inches. I measured the fibres of the left ventricle, and found they had a mean of 8·625. Well, I suppose there is no one in the room who will be able to tell me the ratio of the cubes. I get it out as 2·004. I believe that to be a most remarkable result, and to entitle us to assert that the principle of least action is capable of solving the difficulties of the heart's action, and bringing us to know one more of the many beneficent laws of the all-wise Creator. How it would have rejoiced the heart of the great Kepler had he known this ratio! Divine Geometry! Queen and Mistress and Judge of all Sciences, thy right to rule them shall never be disputed!

This principle of least action applied to the heart consists of simply making every fibre and part of the heart do the entire amount of work of which it is capable. Engineers have admitted the same thing. If you take a fowling-piece, it is of less consequence how the fibres in it are arranged, as it is quite able to resist the explosion for which it is constructed; therefore, no one thinks of asking how its fibres run. But when you come to the construction of 600 lb. guns—such as the Armstrong—then you must calculate your arrangement of the fibres so as to compel every fibre of steel or wrought iron, as the case may be, to bear its own strain. I went down to see this great gun. It consists of eight rings. The first, sixth, and eighth rings were burst, the other five were whole. But a perfect gun would burst so that all the eight rings would give way together, each perishing with its neighbour. And that problem—which engineers have yet failed to solve—is solved in the heart of every person sitting in this room.—*Medical Times and Gazette, June* 10, 1871, *p.* 653.

18.—ON THE ACTION OF THE HEART.

By Dr. J. Milner Fothergill, late Senior Resident Surgeon the Leeds Public Dispensary.

The heart, as has been shown by Pettigrew, consists of several folds of organic fibre-tubing folded on each other; it is thus capable of distension and rhythmical contraction. For this purpose, a singular system of innervation, motor and co-ordinating, is provided. The real motor power of the heart is under the control of minute ganglia, each with a morsel of muscular fibre under its direction, which are alone capable of carrying on the action of the heart, but only in a tumultuous manner, as seen after section of the vagi. As with all other accumulations of organic fibre, the distension of contents leads at length to contraction in a more or less rhythmical manner. The vagus exercises over this a co-ordinating (Von Bezold), or even an inhibitory action; *i. e.*, the application of a stimulus, as electricity, for instance, to the vagus, retards the cardiac contractions, and if the stimulus be powerful enough, arrests the heart's action in diastole. The vagus then normally acts against the first impressions of distension, and only permits contraction when the distension is sufficient to produce uniform contraction, which then goes on in a truly peristaltic manner, but so swiftly as to be easily mistaken for a simultaneous general contraction. Thus, to some extent, distension and the action of the vagus balance each other; any disturbance of that balance, then, would produce irregularity, no matter in what

direction the disturbance might lie. There lies, too, in organic muscular fibre, an inherent power of growth to meet demand; thus, if increased strain be thrown upon the heart, increased growth of muscular tissue, in health, takes place, and again the balance is restored between the blood to be driven and the power to drive it. When, however, from any cause, there is a deficiency in the compensatory nutrition, a species of balance is again struck, but of a lower form, by distension of the fibres, or, in other words, dilatation. In fact, there is planted in this comparatively simple cardiac innervation, a more complex one of contraction and reduction of the ventricular cavity on the one hand, and a species of normal distension on the other. These actions are regulated by nerves, which have been dissected out in the rabbit, and experimented on by MM. Cyon, Claude Bernard, and others. The one which calls into action an increased action against obstruction is called the *accelerator nerve;* and the other, which admits a normal distension of the walls, is called the *depressor nerve* of the heart. Claude Bernard has even gone so far as to state it as his opinion that, through the action of the latter, a species of distension may so take place, in accommodation to existing circumstances, as to convert the cardiac chambers into temporary blood-reservoirs. This their construction as organic fibre would permit; for organic fibre permits great distension without abolition of function. Thus, while carrying on the circulation, by expelling a quantity only of the contained blood off the top of the ventricle, and permitting a large portion to remain on each systole, this accommodation is allowed without bad results. This is undoubtedly no rare occurrence, if clinical observation were exact enough. Thus, between the ordinary balancing powers of the obstacle of the blood to be driven, and the muscular power to drive it, of the stimulating effects of distension in producing contraction, and the controlling action of the vagus, the heart's action ordinarily rocks; but, in addition to that, there is a more complex system of accommodating distension on the one hand, and an accelerating contracting action against an obstacle on the other; which exercise a regulating power according to special circumstances. Ordinarily, however, the action lies between the first set of nerves, with the driving power, and the work to be done. With their disturbances of balance, we are now more especially engaged.

Distension.—The first action of disturbance of this balance is engorgement or distension. When much blood has been located in the ventricles, and they are not capable of completely emptying themselves, a portion remains at each systole. At each diastole, however, an equal quantity of blood is again

thrown into the ventricle, and thus at the next systole a larger quantity remains unexpelled; and this process goes on until death, or until some compensatory relief is attained. This relief is usually attained by congestion of the veins, and the system suffering. This distension or, if chronic, dilatation is produced in many ways, thus:—1. Pouring in of the blood under increased pressure, as in the enlargement of the left ventricle, which follows in time on mitral regurgitation, and increased action of the right ventricle and thickened pulmonary vessels; 2. Muscular failure from defective nutrition, as in fevers, in coronary atheroma, or pericardial adhesion; 3. Obstruction to the flow of blood forwards, as in deposit of fibrine on the semilunar valves, diseased vessels, &c.; 4. Disorder of innervation, as a disturbance of the balance between the sympathetic ganglia and the action of the vagus; 5. Excessive exertion and consequent cardiac exhaustion; 6. Valvular insufficiency. This condition may pass on to permanent dilatation.

Mode of Repair.—The ordinary modes of repair of this condition are two:—first, relief of the condition on which it depends, where practicable; and, second, hypertrophy, when it is due to increased difficulty in the flow forwards, or to valvular insufficiency, by that power of self-increase which is allowed to all muscular fibre, but with which organic muscular fibre is endowed *par excellence.* Restoration of the balance may take place in three modes, of higher and lower grades:—1. The highest, restoration of the cavity to normal size; 2. Hypertrophy, by increase in number of fibres (Forster) and thickening of existing fibres (Bamberger and Rokitansky)—a compromise; 3. Dilatation, a permanent distension—the lowest restoration of balance, and entailing diminished vital capacity to a point proportioned to the heart's lowered power.

The signs of this disturbance of balance, or partial asthenia, are three:—palpitation, irregularity, intermittency.

1. *Palpitation.*—The first evidence of failure of power is palpitation. It is undistinguishable from increased action, except in deficiency of results. When there is excited action, as in exertion or excitement, it is perceptible in the bounding pulse, or, with the sphygmograph, in the increased apex-beat. Palpitation is not so accompanied; and though to auscultation and percussion the heart-stroke may appear identical, it is in the results the real difference lies. Palpitation is a laborious heart-stroke, but not a stronger one. It is the evidence of effort, not of capacity. It is barren in result—a laborious stroke in place of a normal one, but not of more effect, not always of so much. It is intimately associated and commonly mixed up with the next form—irregularity. Palpita-

tion may be engrafted on symptoms of chronic insufficiency as a temporary condition; as, for instance, in the palpitation of Bright's disease, or of slight exertion in a dilated heart from valvular insufficiency. Palpitation is a violent effort of the heart-walls to overcome the action of the vagus, *plus* the obstructed flow.

2. *Irregularity.*—Irregularity of the bulk of blood transmitted into the arteries is one thing, and is due to auriculo-ventricular incompetence. Irregularity in time is another. The latter is under consideration here. Irregularity of rhythm is not due to disordered innervation, but to obvious debility. It is an arrest of the commencement of the peristaltic contraction or heart-stroke. The controlling action of the vagus arrests the contraction until such time as the layers of fibres, acting early on the systole, ought normally to have acted; and then a sharp, almost simultaneous contraction takes place, with an increased thud against the chest-walls. This action is homologous with the increased action of the muscular fibres, under the control of the cardiac ganglia, when the systole has been retarded by stimulation of the vagus. Sometimes it appears as if the action of these layers, acting early in the systole, had been lost; and there is a perceptible change of action in the heart, as if the contraction were commencing from a new layer or set of fibres. Richardson has compared this to a change in order of the number of strikers on an anvil. It always reminds the writer of a horse changing its feet when cantering. This is a more serious evidence of over-taxation of the heart, and is often found mixed up with palpitation. It is often the result of over-exertion, and affords strong suspicion of ventricular distension. Chronic irregularity may have engrafted upon it a passing palpitation from an intercurrent additional disturbance of balance. Nervous irregularity and palpitation will be considered in a future section.

3. *Intermittency.*—This is sometimes purely nervous, and is inexplicable. When persistent, it is usually associated with an impaired first sound, defective apex-beat, and other signs of degeneration. The occurrence of palpitation during this condition sometimes clouds the diagnosis, and makes it very difficult. Intermittency of ventricnlar contraction is not identical with intermittency of radial pulse, though related to it. In some cases of intermittency of the radial pulse, if the stethoscope be applied while the pulse is held, a ventricular contraction will be distinguished when there is no evidence in the pulse; but it is a feeble contraction. In other cases, the ventricle does not perceptibly contract. Whether there is a very abortive contraction, or not, is doubtful. Certain it is, that in some cases the ventricular contraction can be detected when

too feeble to produce a pulse wave; in other cases, no ventricular contraction can be perceived. Intimately connected with this condition, is true angina pectoris; and, finally, cessation in diastole.—*British Medical Journal, July* 1, 1871, *p.* 6.

19.—THE EFFECTS OF OVER-WORK AND STRAIN ON THE HEART AND GREAT BLOOD-VESSELS.

By Dr. T. Clifford Allbutt, Physician to the Leeds General Infirmary.

[The idea is pretty general in the profession that heart-diseases are due, as a rule, in young people to rheumatism, and in old people to atheroma: also that aneurism of the aorta is due chiefly, if not exclusively, to constitutional antecedents. A very slight reference indeed is made to bodily work, as a cause of heart-disease, either in the current teaching or the current text-books of the day. On settling amongst a heavily-labouring population, Dr. Allbutt found a large number of cases of heart-disease in young well-made subjects, of healthy build, and previously unaffected by constitutional disease. Speaking of the literature of the subject the writer observes:]

Infinite pains are given to the pathological anatomy of the heart, infinite pains to the description and classification of morbid sounds. Little, however, is given to the treatment of heart-disease, and little to the investigation of causes other than constitutional inflammations and degenerations. Curious cases are sometimes recorded in which a sudden shock has ruptured a valve; these cases are, however, given as curiosities, and are not regarded as extreme instances of an agency always at work in some degree. One recent and deservedly eminent English author says, indeed, that although examples of valve-rupture are published, yet his belief derived from post-mortem examination is, that rupture of a healthy valve never happens. It does not occur to him that the diseased valve he has before him may owe its disease to the same causes as those to which it owes its rupture. I have recently been made aware, however, that a few writers have had the mechanical causation of heart-disease forced upon their notice in an exceptional way, and these are the medical officers of our army. Their writings are unfortunately hidden away in special reports, unseen by the profession at large; and I am made acquainted with their contents for the first time by the quotations contained in the excellent little book recently published by Mr. Myers, on Diseases of the Heart among Soldiers (Churchill, 1870). This book came into my hands when the present paper was ready for the printer; and it is a peculiar satisfaction to me to find that he

uses almost the same descriptions and arguments in explanation of heart-disease in the army which I used at the meeting of the branch of the Association at Bradford a year before to explain its frequency among forge-men, colliers, wharfingers, and other persons exposed to the heavy strain of town labour. That this experience of Mr. Myers is almost confined to those physicians who practise in the army, is evident from the following passage in Sir W. Jenner's address delivered to us at our Leeds meeting in 1869. His words, which struck me at the time as opposed to my experience, are as follows: "We have attained to this practical conclusion, viz. that, regarded in a clinical point of view, structural changes in the valves of the heart are referable to one of three classes—imperfection in development, acute endocarditis, degenerative changes; and yet farther advance of clinical knowledge has shown that non-fatal acute endocarditis is almost limited to acute rheumatism, and that degenerative changes, sufficient in degree to interfere with function, do not occur in the valves of the heart till middle life, and rarely till advancing life."

Such is the opinion of this most distinguished physician; an opinion founded upon a long experience of the diseases of the upper classes of society, and of the diseases occurring among the patients of a hospital away from forges, engineering-works, docks, and other places of heavy labour. My experience in Leeds, and in a hospital placed in the midst of such works, is almost precisely the reverse. The heart-diseases due to senile degeneration are, of course, as numerous or more numerous than elsewhere, and seem to come on sooner; but the diseases due to imperfect development are rare; and those due to acute rheumatism are, among young men, fewer than those which I have learned to attribute to over-exertion of the body.

It is my purpose in the present paper to describe first the kind of cases which appear to me to be due to mechanical injuries; taking these not in the order in which I discovered them —for of course I discovered the worst kinds before detecting their insidious beginnings—but in their natural order; secondly, I shall try to show how the agency of mechanical causes accounts for these cases; finally, I shall support my arguments by the experience of other physicians, to which I am now enabled to add the farther evidence collected by Mr. Myers.

It has been interesting to me to assure myself that as I studied these cases of mechanical heart-disease they fell into a very natural class, having well-marked characters among themselves, and presenting unlikenesses to other forms of heart-disease. That a certain hypothesis explains a definite class of facts, and segregates them into a natural group, is but another way of saying that it is true. We have now to examine the facts, and to see how far their evidence goes.

The order in which the chronic morbid changes seem to present themselves for consideration is as follows: 1st, dilatation of the right heart; 2ndly, dilatation of the left heart; 3rdly, in reason, if not in time, hypertrophy of the left ventricle, or of both ventricles; 4thly, chronic inflammation of the aorta and aortic valves; 5thly, dilatation of the aorta; 6thly, incompetence of the aortic valves; with, 7thly, farther compensatory hypertrophy of the left ventricle; 8thly, loss of compensatory hypertrophy, with consequent rapid failure, and often with consequent mitral regurgitation.

This order of succession may be complete, or may present these variations among others, that, after the fourth stage, the inner coats of the aorta may and often do give way, and dilatation of the aorta is complicated with saccular aneurysm, or aortic incompetence may take place as a primary or as an earlier event. In the former case the course of events is somewhat variable; but we often find, sooner or later, the sixth and seventh changes in association with it. Sometimes aneurysm precedes incompetence or even simple deterioration of the valves; sometimes it appears at the same time; sometimes, but far less often, it occurs subsequently.

I have been fortunate in keeping a large number of these patients upon my books, of watching them week by week for years, and finally of securing post-mortem examinations of their remains. Autopsies, however, in these as in other cases, illustrate the extreme stages for the most part, and we are left to symptoms, to physical signs, and to reason and analogy, for the understanding of the earlier stages.

With regard to the two first stages, simple dilatation and simple hypertrophy, I have little more than the latter kind of evidence to offer. So constantly, however, do I meet with these states of the heart in patients who have been subjected to over-exertion, that I have no hesitation in placing them together as the beginnings of mischief.

Whether dilatation appear alone, or whether the enlargement consist wholly or in part of hypertrophy, depends apparently on several conditions. If the heart over-taxed be a feeble one, we find dilatation of both chambers; if again the patient, though of strong build, be, through carelessness or poverty, deprived of proper nourishment, we find the same thing: if the lungs be small, we find dilatation of the right heart especially; and this, moreover, we find in cases where the lungs, though adequate in size, are hampered in their movements, as in athletes, or soldiers, tightly clothed about the chest, or in labourers whose occupation consists of work, such as lifting of weights and the like, which fixes the walls of the chest and prevents its full expansion. But if, on the other

hand, we have to deal with the effects of over-exertion under none of these disadvantageous conditions, if the person be well built and well nourished, if his lungs be of adequate size and have free way to expand in a pure atmosphere, we then may find also enlargement of the heart, probably of both ventricles, but we find hypertrophy in proportion to dilatation.

[Prolonged rest is of the greatest service in all these cases—but such hearts seem always to take some time to right themselves.]

In passing on to valvular sufferings we have to inquire in what relation these stand to the dilatation already described. For my own part, I am satisfied that by sheer stretching of the chambers the auriculo-ventricular valves become incompetent. I disbelieve in Dr. King's "safety-valve" hypothesis: I have watched quantities of overworked hearts, and I believe tricuspid regurgitation is as truly a disease as mitral regurgitation, though more common, more easily established, less audible, and, in its early stages, less dangerous. Clinical experience and examination of the dead alike assure me of a fact which many readers I know will doubt—namely, that mitral incompetence may and does result from over-distension of the left ventricle. On the other hand, I have never seen evidence of tricuspid incompetence in hearts assuredly sound, although I have looked for it in scores of labourers and athletes under exertion. Whenever the venous reflux has been distinctly visible, I have been able to recall it quickly in the same persons after long intervals of rest; showing, so far as this symptom has value, that the right auriculo-ventricular orifice is in such persons permanently weakened. Mitral regurgitation is seen less often, because the left ventricle is less frequently over-taxed and is stronger; but out of several cases before me, in which this state of regurgitation was established in the left heart, let me select the following:

Strain on Drill. Dilatation of Heart.—John Rogers, aged 23; shoemaker, of slight build; said to be temperate and to have enjoyed good health. He never had rheumatism in any form, nor does he smoke much. Has been on drill as a militiaman four times. Went up to drill in April 1870, when in good heath. He found the work much harder than before; his clothes were tight, and the cross-belt and straps of knapsack were very confining. He felt this the more, as the drill took place in part up and down a steep hill-side. Many of the men complained of being overdone. He managed to keep up during exercise; but on the first and every subsequent occasion, on coming home and removing his knapsack, he was seized with a violent and distressing fit of coughing, which lasted an hour or more. He never had a cough before. When his time was

over, he felt very short of breath and oppressed in the chest, which symptoms, before unknown to him, have grown worse. On examination in Oct. 1870, the heart-beat is much diffused, rather rapid, and excited. The whole heart is much dilated, its dulness extending nearly an inch beyond the sternum, and its sounds are thin and short. There is a systolic murmur heard at the apex, and less loudly over the xiphoid cartilage.

I was struck on hearing this intelligent man's description of the embarrassment caused by his breast-straps, not only because it bore out my own views of the consequences of deficient pulmonary area or of combustive material upon the right chambers of the heart, but also because it reminded me very strongly of a conversation held about a year ago with my friend Prof. Rolleston, who entered with warm interest into my views about heart-work. He especially urged me to remember the provision made in diving animals such as the whale, which animal does not even wear a breast-bone at all, still less a breast-strap. As in diving, however, there is only need for room to hold the venous blood, and no need for excessive combustion (as in drilling upon a hill-side), the absence of a sternum may be useful in allowing the pressure of the water to squeeze the heart, and so prevent the influx of venous blood. The venous reservoirs in the livers of some seals fulfil the same end in protection of the heart. In John Rogers distention of the heart had resulted in mitral and perhaps in tricuspid regurgitation. Dr. Gairdner has likewise noticed this sequence between the dilatation of the left ventricle and mitral incompetence; and Dr. Bristowe has written in support of the same view in the British and Foreign Medico-Chirurgical Review for 1861, p. 215. Dr. Bristowe, in a paper which is too good to be buried in the back volumes of a magazine, proves, I think, satisfactorily, that the incompetence in these cases is not due to a stretching of the orifice so much as to a deterioration in the muscular and tendinous cords of the valves. This explanation clears up some cases of secondary mitral incompetence which were formerly a puzzle to me, as at the autopsies I had not been able to demonstrate mitral dilatation. I had, indeed, presumed, until I read Dr. Bristowe's paper, that the dilatation was "occasionally" produced during functional activity, and that the orifice might recover itself when the ventricle was no longer distended. Certainly it is true, but I think hitherto not formally recognised, that mitral regurgitation, or at least the murmur of it, is not constant in these secondary cases, but may so completely disappear for hours or days, that when I first studied cases of secondary mitral incompetence following aortic disease, I several times accused myself of error in diagnosis. On my following visit,

however, I should perhaps find that the murmur was re-established, and that the inconstancy lay in the facts themselves, rather than in the inferences. Dr. Peacock, in his Croonian Lectures (Churchill, 1865), also accepts, I think, Dr. Bristowe's arguments, and adds some interesting cases of his own to the same effect. Especially he refers to the great frequency of dilatation of the chambers of the heart in certain Cornish miners, who not only have heavy hammer-work during the day, but whose foolish masters exhaust them farther by allowing them no power but their own to raise them from the pit-bottom when their day is done. An hour of ladder-climbing is often required of them in an evening.

In such a way is incompetence of the tricuspid and mitral valves brought about. Less frequently is it due to sudden violence.

It is in the aortic region that over-exertion nearly always tells, primary injury to the other valves being rare. As by natural selection it is generally the strong men who betake themselves to heavy work, so also it generally happens that the labouring heart grows *pari passu* with the dilating stress upon it, or nearly so. Again, puddlers, strikers, and other men who bring powerful frames into the market, are usually well paid, and their abundant diet and large lungs secure both sufficiency of fuel and sufficiency of combustion. But the severer their work and the more powerful their muscles, the more resistance is there to the flow of blood on the systemic side, and the more tendency is there, not to accumulation of blood in the right ventricle, but in the left heart and aorta. Now the left ventricle can meet this by muscular growth; and I have found in a few autopsies of such men, killed by accident or acute diseases, that the ventricles, the left especially, are, like their bicipites, big and red. Such hearts are quite healthy when seen in an early stage, although the hearts may weigh, as in one instance, as much as sixteen ounces, and may often weigh eleven and twelve ounces.

The case is very different, however, with the aorta: it has no power of strengthening itself according to circumstances. Its resistance is great; but its activity is nothing, or rather it is nothing more than the recoil of elastic fibre. Strain of such coats as these, so far from bringing gain of strength, brings loss of elasticity and weakness; and in the hearts to which I have last referred, healthy as they are, and sound as the vessels may seem yet to be, the aorta is even now beginning to lose elasticity, and to dilate. The inner coats of such aortas, when carefully examined by the microscope, showed in two instances many points of endoarteritis, and a good deal of diffuse granular "exudation" among the fibres of the middle

coat. From this point, pouching of the aorta, with consequent or concomitant incompetence of the valves, is quickly established; and such cases form the staple of heart-diseases in the younger male patients of our hospital. The accumulation of blood in the aorta distends the vessel, and the heart, hypertrophied by this weight above it, beats with a violent pulse below; so that the two causes together stretch the vessel, bulging it in a manner familiar to all pathologists, and producing the miliary lesions in its tissues which are the starting-points of endoarteritis. During the earlier stages, the aortic valves remain competent, the patient presenting himself at this period with a heaving heart, the apex of which is displaced downwards and outwards, and with a rough systolic murmur over the sternum. The valves will still be heard to close, and the sphygmograph will delineate hypertrophy, aortic roughening, without obstruction and with valvular competence. Let this patient come again under notice, however, after a while, and suppose that he has pursued his work, in spite of some paroxysmal dyspnœa, and in spite of a peculiar anæmic state combined with nervous irritability and depression, which often are the only symptoms at this stage,—and we shall find that there is now incompetence of the aortic valves; either because the orifice has been stretched with the pouching of the vessel, or because the valves have given way as a flooring. Now we shall find two rough murmurs over the sternum, the second probably a rasping murmur; there will be more decided dulness upon the manubrium sterni, and the aorta may be felt to beat behind the episternal notch.

The coronary arteries differ from all others in this, that they are filled during the diastole of the heart. To feed them well, we should have the systole of an elastic aorta, and competent valves below it. If either of these conditions fail, we may suppose that the coronary arteries are imperfectly fed. Part of the blood which should be shot into them is shot back into the left ventricle; and again, if the elasticity of the aorta be lost, the blood receives little or no impulse either way, and tends, therefore, to roll all back again into the ventricle by the mere force of gravitation. Even when the valves are competent, as in the case of J. M., a widely dilated and inelastic aorta ill serves its function as a coronary heart, and the ventricles, which should be nourished by them, fade away. The life which was possible to a heart whose additional strength could make up for the disadvantages under which it worked, is no longer possible when the strength of that heart is gone. With wasting of the ventricles, we have wasting likewise of the papillary muscles; and, according to the degree of their wasting, we have the constant or inconstant mitral regurgita-

tion which precedes dissolution. Several of these hearts, and especially the three now lying before me, were watched by me during the greater part of their course. In one case, that in which the mouths of the coronary were made up, I had watched the heart for five years, almost without a break. The man, a most respectable heavily-labouring forgeman, came to me year by year, at the dispensary and at the infirmary, for the steel and digitalis which helped him on. At last he came into the house to die. When I first saw him he had dyspnœa, hypertrophy of the left ventricle, and a soft systolic murmur; then came more evidence of a dilated aorta; and then a diastolic aortic murmur; finally, he lost compensation, the sphygmographic up-stroke fell from its inordinate height to less than the normal measure, the impulse of the heart became feebler and more diffused, and the water-hammering of the arteries subsided. Inconstant mitral regurgitation was heard shortly before death, and he was taken into hospital, where he died in a few days. During these few days no arterial shock was perceptible, and sphygmographic tracing showed that the steady regular pulse of aortic disease was broken up by the irregularities and intermissions of mitral regurgitation. The kidneys, in all these cases to which I refer, were of course substantially healthy; though no doubt in most of them the organs were congested and beefy.

I have made no allusion to the effects of beer-drinking in these cases. No doubt forgemen and other labourers do drink large quantities of beer; but I cannot satisfy myself that any such changes as I have described occur in the innumerable young beer-drinkers of lighter occupations who frequent the hospital. It is in accordance with experience and with physiology to suppose that great quantities of beer are burnt off harmlessly during heavy labour. Teetotal labourers eat largely of hydrocarbons. For the same reason I omit any consideration of the effects of tobacco, which I have carefully eliminated in my consideration of the "irritable" hearts; and of syphilis, which does produce aortic disease; cases of which I shall publish separately hereafter.

The other kind of aortic mischief which results from heavy labour, and perhaps as commonly as the kind already described, is primary aortic regurgitation; not consecutive, that is, to pouching of the aorta. If the aorta be uninjured, a slight incompetence of its valves may long remain unnoticed. When the regurgitation, however, becomes excessive, and the ventricle has to grow greatly, its violent impulse becomes annoying; it begins, also, to set up miliary lesion in the aorta, which was constructed for a ventricle of two-thirds the power; the aorta then pouches, and we fall into the series of events

already described. Why, in some cases, the valves go first, and the aorta subsequently; and why, in other cases, the valves take the lead in mischief, is hard to say. I once strongly believed, and now have some belief, that continuous labour, such as hammer-work, was more injurious to the aorta; and that more sudden strains, like the lifting of weights, told rather upon the valves. Lately, however, I have met with cases which disturb this hypothesis; cases, that is, of valve-mischief caused by continuous over-exertion, and of aorta-mischief, caused by sudden efforts. If, however, one sudden effort be the cause of mischief, we never find pouching of the aorta as a consequence; but we find a crack, which crack may be in the floor of the aorta causing regurgitation, or in the side of it, giving rise to saccular aneurism. Thus it is with single efforts; but, when we come to refine upon labour, and endeavour to separate it into two categories of sudden and continuous efforts, we find very often that the distinction is almost impossible.

[Dr. Allbutt then proceeds to apply the like reasoning to his experience of aneurism, on which point he observes :]

I am satisfied that mechanical causes play a great part in the causation of aortic as well as of other aneurisms. I should have thought it unnecessary to say, that, in claiming attention for the mechanical origin of aneurisms of the aorta, I do not thereby deny a like effect to constitutional degenerations, were it not that such construction was put upon my remarks at the Association meeting at Bradford. So far from denying the constitutional origin of aneurisms, I had two cases of the kind at that time under my care. They were both thoracic aneurisms: the one was in an old man with general arterial disease; the other was in a young man rotten with syphilis. Both cases were at the time constantly pointed out by me as examples of the constitutional origin of aneurism. But what I did say was this,—that we do not pay sufficient attention to the fact, that aortic aneurisms, both thoracic and abdominal, do so frequently occur in men who are young and of healthy tissues. If these patients attribute their disease to a particular strain, the note-taker is as likely as not to set the story aside as immaterial. I contend, that so far from being immaterial, a sudden strain is here not only a cause, but the commonest cause, of aortic aneurism; for in the majority of instances this cause is alleged, it is reasonable, and it stands alone. The inner coat of the aorta is brittle by nature, and on a sudden distension of the walls of the vessel, it may be a trial of strength beteewn this lining membrane and the same membrane on the valves which shall go first. That on the post-mortem the aorta is

found to be "atheromatous," is no ground whatever for the complacent assumption that the "atheroma" is antecedent. On the contrary, the endo-arteritis is as probably a consequence of the same kind as the diffuse mischief surrounding an injury to any other tissue. And as a matter of fact, we find this mischief in abundance immediately about the seat of the lesion; and it dies away gradually until it ceases, no such mischief probably being visible on any other part of the arterial tree. At the same time, as I often discover big hearts, and thick, opaque, but competent aortic valves in heavy labourers who have died of accident or acute disease, and who were not known to suffer from heart-disease; so, under the same circumstances, among our Leeds forgemen, bargees, and the like, I find patches on the inner face of the aorta: sometimes many and large patches; patches, moreover, with unpleasantly-pitted and thin spots in them, which need but little encouragement to give way. One case of this kind in particular I call to mind, where the patient, a powerful bargee, died of pneumonia in the midst of apparent health. Here, I admit, I do more: I claim local degeneration as an antecedent of saccular aneurism, as I have shown it to be an antecedent of pouched aneurism. But that even this mechanical degeneration is not a necessary antecedent, I presume from the occurrence of aneurism from accident in persons not previously devoted to heavy muscular work.—*St. George's Hospital Reports*, 1870, *Vol.* v., *p.* 23.

20.—ON DISTRAIN OF THE HEART.

By Dr. Reginald Thompson, Assistant-Physician to St. George's Hospitol.

Dilatation of the heart seems to be so generally considered as a chronic disease, requiring some time before its presence is indicated by constitutional disturbances, which are insidious and gradual, that a few instances in which this form of cardiac injury arose suddenly, and from an accidental cause, may be worth recording, if only to call attention to a train of symptoms which at first are very puzzling, and liable to mislead the observer, and of which I have hitherto been unable to find any account.

The fact that a heart will yield to long-continued and excessive exertion is, indeed, popularly recognised; and recently attention has been directed to the results of athletic exercises, which bear closely upon this question.

The form of accidental injury which results from excessive pressure on the valve-strings has been sufficiently described. But is it not possible for a muscular organ like the heart to

yield generally and in all directions from the effect of a sudden and undue strain? Or is it the fact that the fibrous coverings of the heart are in all instances a sufficient protection against such an accident? This, I conceive, is not the case. The pericardium, it is well known, can yield to a very great extent, There is a preparation preserved in this Museum which shows the pericardium so distended with fluid as to extend from side to side of the chest, completely masking the lungs when the body was opened. The fibrous investment of muscle can also yield, and that suddenly, as in cases of purpuric hemorrhage into the substance of muscle, which may take place suddenly to such an amount as to form large tumours. It becomes, then, a question as to the time necessary for the production of such dilatation; and as a very short time is required sometimes for the formation of an aneurism of the vessels, so I think the following cases show that a very short time is necessary for the production of an aneurismal or general dilatation of the heart. It is to this form of injury to the heart that I have given the name "distrain," as indicating a special lesion from accidental injury.

The symptoms in these cases are very interesting, as they are likely to draw observation from the injured part to organs remote from the centre of disease; and the patients appear at first to be suffering from intestinal lesion rather than from disease of the heart. The following cases will illustrate this:

A strong well-built labourer, aged 23, was admitted into hospital on the 13th of March, 1868. Six days before admission, after work, he was seized with violent pains below the region of the liver and torminus; but notwithstanding these symptoms, he ate a hearty meal and went to work again. From that time he suffered severe pain, and was brought into hospital in a very precarious condition. He was almost collapsed; cold and clammy; the face blue and congested. He was suffering from orthopnœa: the tongue was dry and furred; he had intense pain in the right hypochondrium below the liver; he was vomiting occasionally. The liver was found to be large; he passed no urine during the time he was in the hospital; the pulse was extremely small and rapid; and the heart-sounds were very hurried and extremely indistinct. I was unable to say if there was any murmur. He had all the aspect of a patient collapsed from intestinal obstruction. At one a.m. on the following morning he was seized with severe cramps in the muscles; and he died at four a.m., after a severe attack of cramps.

At the necropsy the following condition was found. The body was well-nourished and well-developed; the lungs were very much congested and full of black blood; the liver and

kidneys were congested; and the stomach and intestines were found in the same condition. The heart was very large, quite uncontracted, and contained a quantity of scarcely-coagulated blood. Both ventricles were very much dilated, and the auricular ventricular openings much enlarged. The mitral valve was found slightly atheromatous. The muscular structure was pale and fatty. The weight of the organ was 18 oz.

Incomplete as is the history of the case just detailed, yet the sudden access of symptoms, and the general condition of the patient, interpreted and elucidated by the aid of other subsequent cases, leave very little doubt in my mind that the dilatation of the heart, which was apparently the centre of injury, must have occurred suddenly. This view of the case is also borne out by the fact, that the man had been admitted into hospital in the previous January with a slight attack of rheumatism. I had examined him both on admission and on his leaving the hospital, and was unable to detect any disease of the heart.

In the following cases the history of injury is of a more satisfactory kind:

A man, aged 28, was admitted in December with the following symptoms and history. Two weeks before admission he was in good health; but after lifting some heavy weight he suddenly became very ill; and although he did not faint, he sweated profusely, and was seized with acute pain in the region of the heart and down the left arm. Notwithstanding this, he went to work; and continued at work up to five days before admission, when he became very much worse, suffering from vomiting and cardiac pain. He had spit blood since the injury.

On admission on the 29th of December, he was in a very dangerous condition, with a look of much anxiety; in much pain, especially about the abdomen; constantly vomiting and retching, and suffering from intense thirst. The pulse was very irregular and feeble. His chief distress was the abdominal pain. A loud blowing murmur was heard over the base of the heart from the sternum to the line of the left nipple, and more or less over the whole heart's surface; and this organ was evidently very much dilated. A very marked thrill was perceptible between the third and fourth and the fourth and fifth ribs, from the sternum to the left nipple. The apex-beat was between the sixth and seventh ribs. The liver was very large; the belly distended; and the urine was dark and albuminous; pulse 108. He became semi-comatose on the 30th, and made attempts to scratch his heart, as if from pain. He died on the morning of the 31st.

On examination, the organs were found much congested. The heart was much dilated, the ventricles being very much

thinned, and the auricles being even transparent in places; the auriculo-ventricular openings were very much enlarged, the left admitting five fingers, the right six. The structure was soft and fatty. This patient had all the appearance of a man suffering from severe intestinal obstruction. The constant vomiting and retching, and the severe abdominal pain, seemed to point to injuries far from the real centre of disease.

A girl, aged 19, was admitted in February, 1869, with the following symptoms and history. On the morning of the 8th of February she walked rapidly from Fulham to Camberwell, having been previously in health. At half-past one in the afternoon of the same day she was seized with acute symptoms: violent pains in the bowels and back, and frequent vomiting. She did not complain of pain in the region of the heart. She was very ill when admitted: her lips were blanched; her cheeks much flushed; pulse very small and running, 136 in the minute. She suffered from intense thirst, and much abdominal pain and tenderness. Inspection of the chest was sufficient to show that the heart was very much increased in size; its action was perceptible from the third rib; a very distinct thrill was felt on placing the fingers below the second rib. A loud roaring bruit (*du diable*), like the roaring in a shell close to the ear, was heard over a space from the sternum to the external border of the chest. These sounds were also heard to a considerable distance beyond the sternum, to the right side; but they were most distinct at the space first mentioned. A double mitral bruit was heard at the apex. The urine was not albuminous. The condition of the patient was evidently one of much danger; but notwithstanding this, she soon recovered, and went out in a month very much relieved.

She was treated at first with opium, and afterwards with hyoscyamus and belladonna. Under this judicious treatment, and complete rest, the pulse became quiet. On the 16th of February, four days after admission, it was reduced to 120 per minute; and on the 17th still farther to 88. The sphygmograph was applied, and a rapid but regular tracing was obtained. The up-stroke corresponding with the systole was at first very short, but subsequently increased considerably.

In this case the actual damage to the heart was not verified by examination; but the case so closely resembles in symptoms and physical signs those of the two first cases, that I think the same condition of things would have been found.

A few more cases of the same kind have come under my notice, making a total of seven. Of the whole number, three made a good recovery, the constitutional disturbances passing away, although the heart remained permanently injured. Still the organ was able to do its work in such a manner, that the patient considered that health was restored.

As regards treatment, perfect rest is the most important remedial agent; and if I may draw conclusions from the very small number of cases which recovered, it has seemed to me that those narcotics which quiet the action of heart, and tend to reduce the number of pulsations—namely, conium, henbane, and belladonna—are more useful than digitalis, which seemed to me to be of little service.—*St. George's Hospital Reports*, 1870, *Vol.* v., *p.* 119.

21.—ON VALVULAR MURMURS.

By Dr. Henry W. Fuller, Physician to St. George's Hospital.

It is not long since the doctrine was universally taught, that a valvular murmur indicates disease or organic change of an irremediable character in the valvular apparatus of the heart. After a time this lesson was considerably modified, and we were led to discriminate between murmurs dependent on organic changes in the valves themselves and those produced by mere functional causes—by an altered condition or impoverishment of the blood, or by irregular contraction of the vessels. The term functional was still confined to certain soft systolic murmurs at the base of the heart, which could be traced more readily along the pulmonary artery than along the track of the aorta. Of late years observation has led to the belief that the range of the term functional must be greatly extended, and may be strictly applied to murmurs at the apex of the heart, as well as to those already referred to at the base. In many instances of chorea, for instance, a temporary systolic mitral murmur is frequently met with; and the constant concurrence of such murmurs with a disorder characterised by irregular and spasmodic muscular action, led me to suggest that their existence is attributable to some irregular or imperfect action of the valvular apparatus—a view, I believe, which is now very generally adopted. But I am satisfied that the term functional, as applied to valvular murmurs, requires still farther extension, and that cases of functional disturbance of the heart's action sometimes occur accompanied by intense murmur, which in the closest manner simulates serious organic valvular mischief.

Many of you will remember a case in point which occurred in the Queen's ward about two years ago. The patient, a young woman, Mary Ann W., æt. 23, was admitted on March 12, 1868, complaining of severe and incessant palpitation, with consequent dyspnœa. The heart's action was turbulent; its impulse was greatly increased; a loud rough systolic murmur was audible at the apex, and a systolic and slight regurgitant murmur at the base. The impulse of the heart was so violent, its action was so turbulent, and the murmurs were so intense,

that if much febrile disturbance had existed, it would have been difficult to avoid the conclusion that the symptoms were referable to acute endocarditis. For some days the symptoms continued unabated, and the girl's suffering from palpitation was very great; but under the influence of the tincture of the veratrum viride, the palpitation ultimately subsided, the murmurs ceased, the heart's action became quite tranquil and natural, and she left the hospital. On the 22nd of July, 1868, she was re-admitted under the care of my colleague, Dr. Wadham, not this time suffering from her heart. Indeed, the heart's action was quite normal, and the sounds were free from murmur; so that Dr. Wadham could hardly believe that only a few weeks previously the heart's action had been turbulent, its impulse excessive, and its sounds accompanied by loud systolic and diastolic murmurs. However, before she had been long in the hospital, her former heart-symptoms recurred, and with them the same loud systolic and diastolic murmurs, which, as on the former occasion, entirely disappeared before she left the hospital. In private practice I have met with two similar cases, and I suspect they are more common than is usually supposed. Their true character is often overlooked, and doubtless their diagnosis is somewhat difficult; nevertheless, it may be arrived at with tolerable certainty, if all the circumstances of the case are taken into consideration. It has been thought that the position of the murmur itself might afford a clue to its functional origin, for in some instances the murmur has been louder and more distinct over the ventricle of the heart than it has at its apex. But this sometimes holds good of organic murmurs, and therefore is not to be relied on. More certain evidence is to be derived from the position of the heart, and the general history of the case. Though the action of the heart is so forcible and turbulent, its apex beats in its natural position, there is an absence of any history of former cardiac disease, and there is little or no febrile disturbance. These are the characteristic and diagnostic features of the disorder. If the murmurs were attributable to old-standing valvular mischief, the heart would be enlarged, and its apex would be felt pulsating lower than usual in the chest; if it was due to mischief of only a few weeks' or a few months' standing, the apex-beat might not be greatly lower than natural, but there would probably be a history of antecedent febrile disturbance, with dyspnœa, palpitation, and præcordial pain; whilst if occasioned by existing acute disease, the usual train of febrile symptoms would be strongly marked. It is possible to conceive a combination of circumstances which would render a correct diagnosis almost impossible; but ordinarily the features already referred to would at least suffice to excite suspicion as to the

functional nature of the disorder, and should lead to caution in giving an unfavourable diagnosis.

There is another point to which I would refer in relation to valvular murmurs. I allude to the pathological significance of murmurs undoubtedly of organic origin. Briefly, I would have you clearly understand that it is simply impossible from one examination to judge of the pathological significance of an existing murmur. The importance of any murmur is strictly proportioned to the amount of obstruction which the mischief in which it originates offers to the onward current of the blood, and of this no single examination will enable you to judge. The turbulence or irregularity of the heart's action, the force of its impulse, the loudness, harshness, or roughness of any murmur, and its position, whether at the base or apex of the heart, do not, even when viewed together, afford a certain criterion as to the existence of organic valvular disease, and still less do they justify any positive diagnosis as to the extent of that mischief, or any prognosis as to the rapidity with which it is likely to run its course. These are points which can only be judged of by repeated examinations conducted at long intervals. A minute bead of fibrin may be so placed at the edge or on the ventricular surface of the valve, as to give rise to a sharp eddy productive of a loud, rough, and persistent murmur, and yet may not offer any serious impediment to the onward flow of blood; whereas in another instance the valvular apparatus may be damaged in such a way as to cause very little eddy in the current of the blood, and consequently to produce very slight murmur, and yet may seriously obstruct the onward flow of blood, and thus may lead rapidly to hypertrophy and dilatation. Be careful, then, not to be over-hasty in forming a conclusion as to the significance even of organic valvular murmur, and be still more so in expressing any prognosis in the case. Content yourselves by stating that organic valvular mischief exists, but that its real practical significance can only be ascertained by repeated examinations conducted at long intervals. If at the expiration of a twelvemonth from the date of the first examination, you find the heart's action quiet and regular, and the apex-beat in its normal position, depend on it there is no serious impediment to the circulation, however loud and rough the murmur, and your prognosis need not be very unfavourable. This would hold good even more fully if the parts remained in the same condition after the lapse of another six or twelve months. If, on the other hand, the heart were to show signs of enlargement, if its impulse had become more forcible, and its apex-beat lower, your prognosis, however slight and soft and apparently unimportant the murmur, could not be otherwise than unfavourable. Several of the former

class of cases have come under my observation, in which a loud murmur has existed for periods varying from ten to twenty years, and in which even now very slight hypertrophy or dilatation of the heart has occurred.

There is yet another point in relation to valvular murmur to which I would beg your earnest attention. I mean, the degree to which the lesions on which these murmurs depend will some-admit of repair, when the patient is placed under favourable circumstances. I press this upon you the more earnestly, because, from the very nature of the case, you are unable here in hospital practice to observe the fact for yourselves, and you must take it therefore on my authority. Reparative action, in these forms of disease, takes place very slowly, and hospital patients do not remain long enough under inspection to enable you to trace its various steps. But it does take place to a remarkable degree—to a degree far greater than is commonly supposed. Let me give you one example. In the year 1865 I was consulted about a young man, æt. 17, who had suffered from rheumatic fever at the age of 15, and again a twelvemonth afterwards; on which last occasion he was reported to have had inflammation of the heart. When first brought to me, he was suffering severely from palpitation, with a heaving impulse of the heart visible through his clothes, and an extremely loud and rough systolic mitral murmur. His father was aware of his son's condition; for his medical attendant had told him that the heart was irretrievably damaged, and that dropsy was the inevitable result. I so far confirmed this gentleman's opinion as to tell the father that the mitral valve was damaged, and that unless the mischief were to subside under treatment, the case would probably terminate fatally at no distant date. But having said thus much, I told him that as the apex of the heart was beating almost in its natural position, there were fair grounds for hoping that the mischief was not so serious as the physical signs appeared to indicate, and that treatment might be productive of real benefit; and I instanced several cases of partial or complete repair which I had met with in practice. Farther, I entreated the father not to regard the case as hopeless, but to do all that was necessary to give his son a chance of recovery. I explained that complete rest, with freedom from excitement, for two or three years, was absolutely essential, as was also a long-continued course of iron, together with cardiac sedatives; and that throughout the period of treatment the utmost care would be needed to regulate the secretions, and avoid a recurrence of rheumatism, My injunctions were faithfully carried out by both father and son, and my patient was brought to me every three months, that I might caution or encourage him, if encouragement or warning were

necessary, and that I might also note the progress of events. Before the end of a twelvemonth considerable improvement had taken place; and when he came to me last March for a certificate of health, with a view to a public appointment, the heart's action was so quiet and regular, the apex beat so nearly in its normal position, and the murmur so faintly audible, that I verily believe the disease would have escaped detection except under the closest examination.

Let me beg of you, then, not to be over-hasty in hazarding a prognosis when you are called to a patient suffering from valvular disease of the heart. Several examinations, conducted at considerable intervals, will often be needed to determine with certainty whether a murmur is organic or functional; and even when a decision on this point has been arrived at, the true bearing of the murmur, and the practical importance of the lesion which the murmur denotes—the degree to which it interferes with the circulation, and the rapidity, therefore, with which it will induce hypertrophy and dilatation, and lead to dropsy, dyspnœa, and death—are only to be ascertained even approximately by carefully noting the condition of the heart as regards it sounds, its impulse, and the position of its apex-beat, at several examinations, conducted at intervals of three or four months. If after some months of observation, the impulse of the heart becomes more forcible, and the apex-beat lower in the chest, the prognosis ought to be more unfavourable than the mere character of the murmur may have seemed at first to warrant; whereas if an opposite tendency is observed—if at each successive examination the turbulence of the heart's action and the force of its impulse are found to be lessening, and the loudness and roughness of the murmur to be diminishing; and if, farther, the apex continues to pulsate almost in its natural position—the opinion may be given, that the obstruction to the circulation is not great, and probably will not tend, rapidly at least, to a fatal issue; nay more, that if due caution be taken, repair may possibly be effected to a very great extent, and that the patient may live through a long series of years in the enjoyment of very tolerable health.—*St. George's Hospital Reports*, 1870, *Vol.* v. *p.* 1.

22.—SUBCUTANEOUS INJECTION OF ERGOTINE IN HEMOPTYSIS.

By Dr. William Allan Jamieson, Berwick-on-Tweed.

In the July number of the *Edinburgh Medical Journal* for last year, Dr. George W. Balfour recommended the hypodermic injection of ergotine in hemorrhages of various kinds; and I

determined to try it in the next case of hemoptysis which occurred in my practice. The result in the following is, to say the least, remarkable.

A. B., aged 41, a tobacco-pipe maker, a fresh-looking man, and not intemperate, fifteen years ago, about the New-year, fell asleep, when rather tipsy, out of doors, one wet night. He caught cold then, and had a cough ever since, which had not, however, interfered with his pursuit of his trade. On November 20th, 1870, he had to walk quickly up a steep hill, and then stood, when overheated, exposed to more or less of a draught. On the morning of the 21st, while at work, and not using any unusual exertion, he brought up a mouthful of florid blood. This recurred at intervals during the day, and in the evening he called for me. I did not then strip him for examination, as he was somewhat overheated, but at once injected five grains of ergotine (procured from Messrs. T. and H. Smith, of Edinburgh), dissolved in ten minims of distilled water, into the cellular tissue of the arm.

On November 22nd, no more florid blood had come up after the injection, only one or two dark coagula. At the base of the left lung, both anteriorly and posteriorly, there was fine crepitation audible at the close of inspiration, but no dulness on percussion. He felt well, and returned to work, an aperient pill being given to avoid constipation. On the 23rd there was slight return of the hemoptysis; and on the 24th it was again considerable. I therefore repeated the injection of ergotine, enjoining strict rest in bed. The result of the injection was the same; the expectoration of florid blood immediately ceased, only a dark clot or two coming away. Rest was continued till December 1st, when he resumed his employment, quite well, with the exception of the moist sounds at the left base.

He continued well during the severe winter of 1870-71, taking, from time to time, a little cod-liver oil, until May 7th, 1871, when he came to me in great trepidation, the hemoptysis having recurred. While in my house he spat up some bright blood; and I therefore had recourse again to the ergotine. The expectoration of bright blood at once ceased, and a tickling cough which preceded it disappeared also. Next day I found him well, only one or two brownish clots having come up. The crepitation was still audible, though less clearly, at the left base; and though there was no appreciable dulness, there was slight flattening of the chest-wall. I ought to have mentioned that I examined the expectoration in November, in the manner recommended by Dr. Fenwick, without finding any fragments of elastic tissue.

Should the result of this case be borne out by further expe-

rience, we shall have to thank Dr. Balfour for introducing a remedial agent sadly wanted, an efficient and rapidly-acting styptic in hemoptysis. No local effect, save slight irritation of the skin, lasting a few hours; and no influence on the pulse, which remained at 72, resulted from the injection.—*British Medical Journal, June* 3, 1871, *p.* 587.

DISEASES OF THE URINARY ORGANS.

23.—ON DIABETES.

By Dr. W. R. Basham, Physician to the Westminster Hospital.

Though physiology has obtained considerable insight into the source and formation of sugar or glucose in the organism, yet the pathology of diabetes is still far from settled. No final or undeniable explanation has been offered which satisfactorily accounts for the large amount of sugar daily formed and passing through the kidneys in a case of diabetes. It is not the error or disorder of a single organ or of any one function which will explain this deviation or exaggeration from the processes of health. The vice or error is probably in many organs and many functions. It may be instructive to draw your attention to the leading hypotheses which seek to account for the excessive amount of sugar formed.

In studying the symptoms of diabetes you must not overlook the physiological fact that sugar or glucose is in small proportion a natural production in the organism; and that it exists not only in the liver in health, but that it is formed out of every article of farinaceous and amylaceous food we eat, and that a small quantity exists even in our nitrogenous food. So that in health sugar is always being formed; but it is only in a transitional state; passing out from the liver by the hepatic veins, it enters the respiratory circulation, where it is rapidly and completely oxidised, and converted into carbonic acid and water; and thus adding to the sources of vital heat, it disappears from the organism almost as soon as formed.

As far as our present knowledge enables us to judge, diabetes is an error of function rather than an organic disease. The organic diseases with which it is occasionally associated are accidental complications rather than essentials to its existence. Tubercle, and cataract, and carbuncle, are far from being present in every case, although not unfrequently accompanying it.

The excess of sugar passing through the kidneys in diabetes

may be accounted for:—1st, either by supposing an exaggeration of the gluco-genetic function of the liver; or, 2ndly, by a defect in the gastric digestion by which the farina of the food, having been converted into glucose by the salivary and buccal and pancreatic secretions, suffers no further change in the ultimate processes of assimilation, but is excreted with the urine as sugar. These views lead up naturally to the supposition of an error in the respiratory function by which the glucose circulating through the lungs is not further oxidised and converted into carbonic acid and water. But it may be that the sugar is formed in such abundance, or in such excess, that the capacity of the respiratory apparatus is not equal to the task of the conversion of so large a supply as is cast upon it.

There are five predominant theories which endeavour to trace or account for the excess of sugar formed in diabetes.

First.—Bouchardat's theory. The amylaceous or starch elements of our food are converted into sugar by several animal products. The saliva, the pancreatic secretion, the gastric fluid rendered alkaline (having lost its property of dissolving flesh), continues capable of converting starch into glucose. He considers the sugar of diabetes to be principally, if not entirely, formed from the farinaceous series of foods. He thinks diabetic persons digest these articles of food differently from healthy persons.

Secondly.—Claude Bernard's discovery of the sugar-forming function of the liver has led to the opinion that diabetes consists in an over-activity of this function, and there are many who, following these views, consider the liver as the chief, if not the only, organ at fault. But this over-activity of the gluco-genetic function of the liver is supposed to arise from some undue excitation of the great sympathetic system of nerves.

Thirdly.—A general and irritable state of the nervous system being a frequent, if not a constant, symptom in diabetes, and as Claude Bernard records that irritation of the ganglionic centre in the medulla oblongata, exercises a special influence over the functions and actions of the liver, and, moreover, as Flourens has shown, that irritation of the floor of the fourth ventricle led to the presence of sugar in the urine of the animal operated on, it has been conceived that the nervous centres are remotely the cause of this singular disturbance in the functions of the organism.

Fourthly.—It has been held by some that a defective respiratory process, by not consuming the elements of sugar in the lungs, might account for the accumulation of the sugar

in the blood and its presence in the urine. M. Reynoso affirms that in almost all cases in which the function of respiration is impeded, sugar appears in the urine. Patients under the influence of ether or chloroform, in the advanced stage of tubercle, in pleurisy, in asthma, hysteria, and epilepsy, in experiments on animals prevented from breathing freely, sugar was universally present in the urine.

Fifthly.—The importance, the absolute necessity for the presence of certain alkaline salts in the blood, without which the further metamorphosis of many excrementitious substances would be incomplete, is well known. M. Mialhe has proved, by the analysis of the blood in diabetic patients, that it is deficient in these alkaline salts; he states that the farina of our food is converted with equal activity into sugar both in health and in diabetes. But in health the sugar is decomposed or burnt off by the presence of these alkaline salts by the agency of the oxygen in the lungs; but in diabetes the decomposition does not take place, owing to the deficiency of these important constituents, and the sugar undergoing no further metamorphosis, passes off as such through the kidneys.

Such are the prevailing theories or doctrines explaining the cause or tracing the source of the sugar in diabetes.

It is absolutely necessary that you should be familiar with these doctrines, for without such knowledge the treatment of diabetes will be incomprehensible to you. The several plans of treatment are based on one or other of these views; and the object for which, or the therapeutic value of any remedy employed, must be referred to one or other of the theories just enumerated.

It is practically of importance in the management of a case of diabetes to ascertain what is the relative amount of sugar derived from the food, and what formed irrespective of the ingesta. You should know that the amount of sugar is always augmented after a meal. The sugar then should be estimated by an analysis of the urine passed from one to two hours after the principal meal; and also by an analysis of the morning urine, or that passed after fasting from eight to ten hours. From the first may be approximately estimated the sugar derived directly from the food; from the second analysis, the amount of sugar will represent the exaggeration of the liver function. The practical deduction from which will be, that diet, or the abstinence from farinaceous foods, will, in a great degree, moderate the proportion of sagar after a meal, but will exercise a very limited influence over the amount representing

the sugar derived from the liver, to control or moderate which will require the use of medicinal agents, the action of which will be explained hereafter.

The case which I select to illustrate these general remarks on the disease is that of a woman, S. D., 64 years of age, the mother of eight children, six of whom, however, were still-born. Two are grown up and living. She stated that her general health, up to within the last twelvemonth, had always been good. The first indications of failing health appear to have been a general physical debility or incapacity for bodily exertion, associated with a very rapid loss of flesh, intense thirst, and a desire for food, which no quantity seemed to satisfy. On her admission the following symptoms were most worthy of note:—There was a great amount of emaciation, the skin being everywhere harsh, and dry, and puckered; not a trace of subcutaneous fat could anywhere be felt. The tongue was moist. The bowels were very sluggish, the dejections being hard, dry, and pill-like. The breath was aromatic. The pulse was small and frequent, with occasional intermissions. The perceptions of sense were typical. The sight was occasionally obscured by musci volitantes; there were no indications of cataract. The sense of smell was not affected. That of taste was, however, disordered by the sense of constant sweetness, which provoked thirst, and tainted everything she ate. The cutaneous sensibility was the seat of irritation about the vulva and meatus urinæ. The chest was free from any symptom of tubercular disease. The urine was passed in great abundance, from fourteen to sixteen pints daily. It was nearly colourless, but, notwithstanding, had a specific gravity of 1034. The copper test gave marked evidence of sugar, and the fermentation test proved that it contained nearly thirty grains of sugar to the ounce of urine. She was at once placed on the diabetic diet, excluding, as far as possible, farinaceous articles, and supplying vegetables from the cruciferous family, supplemented with a large amount of animal food.

The medicinal treatment should in the commencement be governed a good deal by the complications which may attend the case. If there be much irritability and restlessness in the nervous system the treatment by opium in large and repeated doses should be at once commenced. If pulmonary symptoms give ground for the suspicion of tubercle, cod-liver oil and ferruginous preparations should be given. In the case under consideration the symptoms of chief note were those derived from the abundant formation of sugar—the emaciation, the thirst, and the craving appetite, the essentials to the diabetic state. It was a case which seemed to promise benefit from what is sometimes called the alkaline plan of treatment, but which

might be more correctly designated as the ammonio-saline system. It is based on the views already explained of the existence of a deficiency of those salts in the blood. The saline mixture is composed of carbonate of ammonia, ten grains; phosphate of ammonia, ten grains; the bicarbonate of soda, ten grains. These salts, dissolved in an ounce of water with a few drops of the tincture of ginger, are given three times a day, in a state of effervescence, with a tablespoonful or more of fresh lemon juice. Such a medicine is very grateful to the patient; it relieves thirst, and usually, in a short time, mitigates the morbid appetite. The clinical report states, ten days after this treatment had been continued, that the thirst and appetite were much diminished. The tongue was moist. The glutinous sticky feeling of the mouth was gone. She stated she felt more energy in her; less of that physical feebleness which she formerly suffered. By the end of the third week the urine was reduced in quantity, about eight pints in the twenty-four hours.

The remedy was continued for a further period, and at the end of the sixth week, the amount of sugar per ounce was reduced to six grains. The daily amount of urine was estimated as from six to eight pints. The tongue was moist, the thirst by no means urgent. The dejections were no longer hard and pellet-like, but soft and pultaceous, and, moreover the patient had more than once felt some moisture on the skin, which had not occurred for many months. The report to the middle of March continued very favourable; in the third week of that month the thirst and morbid appetite was stated to be much diminished; the urine had decreased to no more than four pints in the twenty-four hours, but the amount of sugar remained the same. I think you may trace all these favourable symptoms to the agency of the ammoniacal saline which has been taken without intermission from her admission into the hospital. It is true the disease has not been cured, but the disorder has been brought within such limits that the more distressing symptoms have disappeared. Before she leaves the hospital the remedy she has taken so long will be discontinued, and after a few days interval, the urine well examined for quantity of sugar present, and observations made to ascertain if the effects of the treatment have been permanent or temporary.

[Dr. Basham determined to test the effect of the ammonio-saline treatment of diabetes by discontinuing it for a few days in this case. No test could be fairer.]

She discontinued the medicines on the 5th April; the diet was strictly continued. On the fourth day of the interval she complained of missing the medicine. She stated that the

clammy, thirsty feeling in the mouth had returned, that her food did not satisfy her as when taking the saline. The amount of urine was daily increasing ; its specific gravity had become 1·038, and the amount of sugar proportionately augmented. There was an evident return of all the leading conditions of the diabetic state on the discontinuance of the ammonio-saline. After an interval of ten days, to be certain that this recurrence of the more prominent and characteristic symptoms was not accidental, the carbonate and phosphate of ammonia were resumed in the form described in the last lecture. If, again, a mitigation of symptoms followed, if the urine and sugar decreased and the thirst became modified a few days after the operation of the remedies, it could not be reasonably asserted that these agents were inactive, or of no therapeutic value. Five days after the resumption of the medicine, the specific gravity of the urine had gone down to 1·027, and the proportion of sugar to the ounce had decreased to ten grains. Other symptoms had again disappeared ; there was little or no thirst, the tongue was not clammy, the breath was inodorous, and the appetite nothing inordinate. You must recollect, however, that it is by no means an unusual occurrence in diabetes in the later periods of life, for the symptoms to oscillate between great extremes, irrespective of whatever remedy may be employed, so that in the view of some this decline of the more distressing conditions might be considered as the natural law of the disease, apart from any therapeutic agency. It is doubtless true that in what has been termed senile diabetes these variations are frequent. But it is not a little singular that the abatement of the symptoms always correspond in this case to the operation of the salines, and the aggravation of them to the interval in which they were discontinued. The same results have so constantly been observed by me in the treatment of diabetes in another class of practice, that I have no hesitation in placing the greatest reliance for the mitigation of the more distressing symptoms, particularly that of thirst, to the beneficial action of the ammonio-saline treatment. It must be admitted that there are cases of diabetes in the treatment of which all that can be hoped for is mitigation, not cure. Reference to the preliminary remarks I made in a former lecture will prove that it is not difficult to diagnose such cases, to determine the fact at the commencement of the treatment how far it will probably be curative, how far only alleviative. This can and ought in every case to be determined by an analysis of the urine, collected at definite periods of the day, having reference to the time of taking food. If the morning urine, passed after an interval of some eight or ten hours' fasting, contains but little sugar, perhaps only a trace; if the urine passed some

hour or two after breakfast or dinner, no rigid abstention of diet having been observed, present evidence of a well-marked notable amount of sugar, the inference should be that the sugar was derived from the farinaceous elements of food, that it was therefore an error of digestion in some of the primary stages, rather than implicating such organs as the liver, lungs, or nervous system as its causes. On the other hand, where the evidence of sugar in the morning urine (*urina sanguinis*) is as great as that of the urine of digestion (*urina cibi*), the disease has a more remote origin, the digestive function is in error, but so is probably that of liver, lungs, or ganglionic system. In the first type of cases the more distressing symptoms of diabetes—unsatisfied appetite, emaciation, restlessness, are absent, or only present faintly, and not distressingly; in the second type of case all of these, with others, are present in aggravated form. Diet and medicinal treatment will cure the first, but diet, however strictly regulated, or remedies however appropriate, will do no more than alleviate the second, for under no circumstances is the disease in this form otherwise than incurable. You will easily perceive, therefore, how desirable, not on scientific grounds only, but on purely practical grounds, how imperatively necessary it must be in each case to determine to which of these two types of the disease the case under your treatment may belong. The mitigation of the more urgent symptoms on each occasion, a few days after resuming the remedies, left no reasonable doubt of the therapeutic influence of the ammonio-saline plan of treatment. Under these agents the patient is increasing in weight, is gaining week by week an increase of bodily vigour, and losing all the prominent and more distressing symptoms of the diabetic state. You will find it convenient in practice in private life to keep, in a tabular form, a list of the articles of diet, which, in the dietetic treatment of diabetes may be allowed, and those which are forbidden; a list also of the wines and beverages which may be taken or avoided is also useful. The following list fulfils these objects:—

Diet Table in Diabetes.

General rules to be observed by those suffering from Diabetes, who should earnestly recollect that an accurately observed diet is more effective in relieving the disease than any other known means.

VEGETABLE FOOD.

Articles of Food Forbidden.	*Articles of Food Allowed.*
Every form of vegetable food containing Flour (Farina), Starch, Dextrine, or Sugar	All the Cabbage Tribe (*Cruciferæ*) Cabbage, Cauliflower

All articles made from Wheat Flour—
Bread, Biscuits, Pastry.
Oatmeal,
Peas, Beans, Haricots, Lentils
Rice, Sago, Tapioca, Arrowroot
Semolina, Revalenta
Potatoes, Yams
Carrots, Turnips, Radishes
Parsnip
Macaroni, except with Cheese
Vermicelli
Cocoa, Chocolate
All Fruits rich in Sugar

Broccoli, Borecole
Scotch Kale, Brussels Sprouts
Sea Kale, Cardoons
Couve Tronchuda (Portugal Cabbage)
Spinach, French Beans
Artichokes
Cucumber, with Oil
Asparagus
Onions, Leeks
Mushrooms, Truffles
Lettuce, Endive, and
all varieties of Cress
Italian or Corn Salad
American, and Water Cress
All kinds of Acid Fruits
As a substitute for Wheaten Bread—
Gluten Bread
Bran Bread
Bran Biscuits
Brown Bread, cut thin and toasted, may be allowed

ANIMAL FOOD.

Articles of Food Forbidden.

None.

No article of food derived from the Animal Kingdom is forbidden in Diabetes.

In every kind of made dish prepared from Animal food, the sauce or gravy, if needed, must be thickened with either Gluten Flour or Egg Powder.

Articles of Food Allowed.

The Food of the Diabetic Patient should be as much as possible selected from the Animal Kingdom.
All kinds of Meat
All kinds of Game
All kinds of Poultry
All kinds of Fish and Crustaceæ
Eggs of every variety that are edible
Cheese in every form and variety
The Curds of Milk.

DRINK.

Forbidden.

All varieties of Beer—
Ale, Stout, Porter
Sweet Cyder
All Home-made or Sweet Wines

Allowed.

All the Bordeaux Wines
St. Julien
Medoc
Lafitte
Sauterne, &c.

All Wines containing a per centage of Sugar or Dextrine—
Port,
Sherry, Brown or Golden
Madeira
Champagne, if sweet
Sparkling Moselle
Sparkling Hock
Sparkling Burgundy

All the Rhine Wines
Every variety of Hock
Marcobrunner
Rudesheimer
Steinberger
Red Assmannshauser
Spirits—Brandy (Cognac)
Whiskey, Gin unsweetened
Rum, Hollands
Tea, Coffee, without Sugar, but with Cream *ad libitum*
Soda, Seltzer, Vichy, and similar Waters.

The whole surface of the body should be clothed with flannel winter and summer.—*Medical Press and Circular, May* 3 *and June* 21, 1871, *pp*. 373, 523.

24.—ON THE RELATIVE INFLUENCE OF BREAD, HONEY, AND SUGAR UPON THE AMOUNT OF UREA AND SUGAR EXCRETED IN DIABETES.

[By Dr. W. WADHAM, Physician to St. George's Hospital.

[The following observations upon the amount of sugar and urea excreted by diabetic patients during the consumption of regulated quantities of bread, honey, and white sugar, were made upon three patients who were under Dr. Wadham's care in the hospital, in the year 1870. The facts which appear to have been proved by the first experiment, made upon the urine of a man 50 years of age, were]

1. That, irrespective of any change of diet, the amount of urine, urea, and sugar excreted in diabetes varies very greatly from day to day.

2. That the excess in either of these constituents does not appear to be accompanied by a decrease in the other.

3. That the addition of honey to the diet causes an immediate rise in the quantity of urine, urea, and sugar excreted, the rise in all these becoming greater as the honey is continued.

4. That only about half of the sugar given in this form appears to be eliminated by the kidneys in the form of sugar, the remainder being probably burnt off in the lungs or assimilated to the system.

5. That whatever truth there may be in the asserted benefit derived from the dietetic use of honey in diabetes, in this case it certainly did not act beneficially by diminishing the amount of urea; for not only was the amount of urine, urea, and sugar

greatly increased during its consumption, but after it was omitted all these remained higher than they had previously been, the increase being especially noticeable in the amount of urea.

[The second experiment was made upon an unmarried woman 26 years of age, who was known to have been suffering from diabetes during the eight preceding months.]

The effect of administering 42 grammes of starch was apparently to convert a case which was daily improving into one which, judging by the amount of sugar and urea excreted, became rapidly and persistently worse. The facts which these experiments seem to prove are the following:

1. That, as was shown by the former case, the amount of urine, urea, and sugar varies greatly from day to day in diabetes, irrespective of any alteration of diet; and that the excess in either the sugar or urea is not compensated by a decrease in the other.

2. That, contrary to that which happened in the former case, a large quantity of honey given during two consecutive days was followed at first by a considerable reduction in the amount of urea and sugar excreted; but that as the honey was continued, an increase in both these constituents took place, and was almost equally evident in the urine collected during the twenty-four hours immediately following the omission of the honey from the diet. The absolute amount of sugar and urea passed during these three days, however, very slightly exceeded that passed during the three days previous to the honey being given.

3. That twenty-four hours after this consumption of honey had ceased, the amount of sugar excreted began to diminish and become gradually less; and although during the first day some diminution occurred in the amount of urea excreted, this was not permanent, as for two days after the honey was omitted it increased enormously—apparently from some transitory effect produced upon the system. The gradual diminution which afterwards took place in both the amount of urea and sugar seemed to show that this experiment had done no permanent harm.

4. That the effect of a small addition of bread to the diet caused an immediate and considerable increase in the amount of sugar excreted; but that the injurious effect of the bread was not fully established until the second day of its administration, after which it seemed to produce a steady increase in the amount of sugar.

5. That the additional amount of sugar excreted when the bread was taken was greatly in excess of that which could have

been formed by the mere chemical conversion of the starch contained in the bread into sugar; and was therefore probably the result of some injurious effect produced by the bread upon the system.

6. That the administration of bread caused an immediate increase in the amount of urea excreted, but that this increase was not, like that of the sugar, progressive, or afterwards very marked.

Finally, honey appeared to produce a favourable effect upon the progress of this woman's disease, bread a decidedly injurious one.

[The third series of determinations were made upon the urine of a man 25 years of age, who had been the subject of diabetes four months.]

The principal additional fact which the experiments in this case seemed to prove was, that the increased excretion of urea following the addition of bread to the diet may be delayed for twenty-four hours; and that when this is omitted from the diet, a similar delay may occur before any diminution takes place in the amount of urea excreted.

The fourth series of experiments were made upon Thomas Jones after his return from Wimbledom. Whilst in the Convalescent Hospital there, he was placed under no treatment, was allowed the ordinary diet supplied to the inmates of that institution, and took exercise freely in the open air. The results of this were, an apparent improvement of his health, a slight increase in his weight, but a decided exaggeration of sugar excretion. So much was this the case, that on the first day after his readmission the amount of sugar passed amounted to 334·55 grammes in the twenty-four hours, or to nearly three times the quantity he was passing when he left the Hospital five weeks previously.

For three days after his readmission he was allowed twelve ounces of bread daily, but no other amylaceous food, no saccharine matter, and no beer. Even under this diet the amount of sugar was rapidly diminishing, when for the next three days he was entirely deprived of all amylaceous food, and then made the subject of experiment. During this his second residence in the Hospital he received no treatment beyond being strictly dieted in the intervals of the experiments. He, however, improved very much in health, and increased steadily in weight, during the whole time, gaining about one pound each week. The time he complained of feeling most unwell was when for a few days he was taking bread; and although after this the amount of sugar in the urine never again fell so low as it had previously been, the average amount of it for several

days before he finally left the Hospital was about one-half of that he was passing when previously discharged, and less than a fifth the amount he passed the day following his return from Wimbledon.

The experiments were three in number, and were made with a view of ascertaining the relative influence of equal weights of honey, bread, and white sugar upon the amount of urea and sugar excreted, and the amount of each of these materials given was eight ounces. It was originally intended to give each of them for a week in succession, and to allow a week to elapse between each experiment, in the hope that by so doing the urine during this interval would return to its previous standard, and lose all trace of the disturbance to its composition caused by the preceding experiment. This plan, however, was not strictly adhered to, and the bread was only continued for four days. The man almost immediately he took it complained of the distressing thirst it caused him; and on this account, and because it inconveniently increased the amount of urine he was passing, he, unlike diabetics in general, was very unwilling to take it. It was therefore thought advisable not to continue this experiment; partly from a conviction that the bread was really doing him harm, and also from a fear that he might employ deceit and not eat it, though professing to do so, and thus destroy the value of the experiment.

[From a fourth series of experiments the conclusions were:]

The first fact worthy of notice is that scarcely any diminution of sugar-excretion occurred during the first twenty-four hours following the total withdrawal of all amylaceous and saccharine materials from the diet, and that very little increase of it occurred from the addition of honey to it, either at once or when this was continued, as it was, for some days. Similar circumstances had been noticed during the experiments formerly made upon this man; for both, when additional bread was given him, and when this was afterwards withdrawn, no change in the amount of excreted sugar took place during the first twenty-four hours. If we calculate the daily average of sugar passed by this man during the week he was taking for two days 142, and for five days 284 grammes of honey, we shall find that it varied so little from that which was afterwards excreted when the honey was no longer taken, that we can only conclude that this latter had in this case little or no influence over the excretion of sugar, the amounts being respectively 195·61 and 184·01 grammes.

Even the addition of 284 grammes of white sugar, which was continued for the next five days, appears to have had but little influence in increasing the amount of sugar, though it

slightly diminished the amount of urea excreted, the average increase of sugar being only 45·47 grammes, and the diminution of urea 2·58 grammes. Thus, in a case of confirmed diabetes, in so advanced a stage that the man died within a month of the experiment, we have the somewhat extraordinary fact, that the addition for five days of 284 grammes of pure white sugar to the diet only caused an increase of 45·49 grammes in the amount of sugar excreted with the urine.

After the white sugar was withdrawn from this man's diet, the amount of excreted sugar diminished gradually, but slightly, for three days. Diarrhœa then occurred, and this was followed for some days by a much-diminished excretion of sugar. At this period the experiment was made of giving 284 grammes of bread with the diet. This caused an immediate and large increase of sugar-excretion, which continued the next three days, and was also accompanied by an excess in the excretion of urea. Directly he commenced this diet his appetite failed, he lost flesh rapidly, began to feel weak and exhausted, and was evidently rapidly sinking when his friends removed him from the hospital. During the time he remained under observation, determinations were still regularly made of the amount of sugar and urea passed, the latter of which was much increased in quantity. These may be seen by reference to the table, but I have not attempted to draw any conclusions from them. At the time it was thought that this man's death was hastened by the administration of bread, and that until this was given there was no reason for supposing that the case was progressing unfavourably. Whether this was the real cause of his rapid change for the worse it is of course impossible to say; but this, and all the other experiments in which bread was given, certainly impressed me strongly with an idea of its injurious influence in cases of diabetes.

I can only regard the results of these experiments as affording us so many unconnected facts from which it would be hazardous to draw any positive conclusions; and I am quite prepared to see many of them reversed by a repetition of the same experiments upon other individuals. It is, then, without any wish of attaching to them greater importance than they merit, that I offer them as a contribution towards our knowledge of the dietetic treatment of diabetes.

If they prove anything, these experiments certainly seem to show that the amount of sugar excreted in the urine is far larger after the consumption of a given weight of bread than it would be after the same amount of honey, and after this latter than after pure white sugar.

Whenever bread was eaten, it always gave rise to a large increase in the amount of excreted sugar; and in one case, or

it may be at one stage of the disease, this increase was absolutely in excess of that which could have arisen from the mere conversion of the starch contained in the bread into sugar. In some experiments this sugar formation was delayed for twenty-four hours after the bread was eaten; and when this was removed from the diet, a similar interval elapsed before, by diminished excretion of sugar, the system gave evidence of its having been relieved from the effect produced upon it by the bread; though, in similar experiments repeated upon the same individual, this result was not constant. The addition of bread to the diet seemed also invariably to increase the amount of urea excreted.

In no instance when honey was given did it lead to a greater excess in the sugar excretion than would represent one-half the weight of the sugar given in the form of honey. In two experiments it was even difficult to detect any very conclusive evidence of the excreted sugar having been increased by the addition of honey to the diet. In other instances in which an increased excretion had taken place, this seemed to continue to quite as great an extent for two or three days after the honey had been entirely omitted from the diet. In some instances, honey appeared to increase the amount of urea excreted; but more generally it diminished this slightly so long as it was continued in the diet. Whenever honey was given, and then omitted, an excess in the excretion of urea followed, or continued with oscillations, for several days. This had the appearance of being the normal excretion, which, having been checked by the presence of the honey, made its appearance in the urine immediately the honey was withdrawn from the diet. The most unexpected results, however, were those which followed the addition of sugar in large quantities to the diet. These results were similar in both instances in which the experiment was made, though one was made on a comparatively slight case, progressing favourably; and the other upon a severe case, rapidly tending towards a fatal termination.

In the first case, the addition of 284 grammes of white sugar to the diet raised the amount of excreted sugar 46·54 grammes, and diminished the urea 5·94 grammes. In the second case, the same weight of sugar raised the amount of excreted sugar 45·47 grammes, and diminished the excretion of urea 2·58 grammes. One of these experiments was continued for six and in the other for five days; and in the former, when sugar was no longer given in the diet, an increased excretion of urea took place, and was still rising when the determination was omitted, on account of the mercury solution being exhausted.

The practical conclusions which the facts above related, if subsequently verified, would seem to justify, are:

1. That in all cases and in every stage of diabetes, bread, and probably all other amylaceous food, should be strictly excluded from the diet; for, if given, it will largely increase the amount of urine, urea, and sugar excreted, and in every way aggravate the symptoms of the disease. It is, however, probable that its injurious effect is less felt by an individual who is at the same time taking exercise and much in the open air.

2. That honey may often be advantageously used as an article of diet, because in some cases, or possibly in some stages of diabetes, a large amount of it may be eaten without materially increasing the weight of urea or sugar excreted; and because, although in other cases an increase of the sugar may occur, this is accompanied by a diminished excretion of urea, and is often very much less in amount than would be represented by the sugar consumed in the form of honey.

3. That pure white sugar may be added to the diet in diabetes with every prospect of a beneficial result; for its use is accompanied by a diminution in the amount of urea excreted, and, when given in large quantities, less than one-sixth of the amount escapes as sugar in the urine, the remainder being either burnt off, or otherwise appropriated to the uses of the system.

As regards the various theories respecting the nature of diabetes, these experiments seem to prove, that it does not at any stage depend either upon a simple arrest at the stage of sugar in the chemical changes which starch undergoes in the system, or upon sugar as it passes through the body escaping those chemical transformations by which, in health, it is converted, first into vegetable acid, and then into carbonic acid and water.

The facts that an interval of twenty-four hours may sometimes elapse before any increased excretion of sugar follows the increased consumption of starch, and that this may at other times give rise to an increased excretion in excess of that which would represent the conversion of the starch into sugar, and especially that a very large amount of pure sugar may be taken in the diet, of which only a very small proportion subsequently appears in the urine, are all circumstances which seem to make it far more probable that the disease depends upon some derangement of the liver, leading to an excessive secretion of sugar by that organ. Upon this theory we can explain the injurious effects of starch, by supposing it to act in some manner directly upon the liver, and are able at the same time to understand why both honey and sugar are always less injurious, or even positively beneficial, when contained in

the diet of those suffering from diabetes.—*St. George's Hospital Reports, Vol.* v, 1870, *p.* 193.

25.—THE CHEMICAL RELATIONS OF URATES AND PHOSPHATES.

By Dr. A. W. Barclay, Physician to St George's Hospital.

[There are many considerations involved in the deposits of urates and phosphates, which are of great service in practice. They guide us in the selection of remedies, and not unfrequently point more or less distinctly to conditions in distant organs, of which the functional disorder of the kidney is only an index and a consequence.]

The rule for the discrimination of the urates and the phosphates is so well known, so universally admitted, and so simple, that an exception might be supposed impossible. The urates are dissolved by heat, the phosphates by acid. A precipitate in acid urine soluble by heat would consist of some compound of uric acid; a precipitate in alkaline urine insoluble by heat would be an earthy salt, and probably a phosphate of lime. Conversely, a precipitate of urates ought not to exist in hot urine; a precipitate of phosphates should not be found in acid urine.

It is very many years since, as a student at St. George's Hospital, I submitted to Dr. Bence Jones, so well known as a chemist, who was at that time one of our teachers, the puzzle, which I cannot say is even now quite intelligible to me, how a phosphatic deposit could be found in decidedly acid urine. But until recently I never met with an instance of urates insoluble by heat. On May 10th, T. S., aged 17, a porter, was admitted under my care at St. George's Hospital, in a state of considerable depression, with a quick pulse, a chapped and furred tongue, and persistent diarrhœa of some days' standing, and the suspicion of enteric fever seemed rather confirmed by slight elevation of temperature. To my own mind, the history was opposed to this idea. It seemed rather to have been a case of severe diarrhœa, in which the prostration was a mild form of collapse, and the further progress of the case bore out this diagnosis. The various secretions were naturally watched, and the urine was exhibited to me with a turbid, flocculent deposit, which my clerk pronounced to be phosphates, because the urine was nearly neutral, and the precipitate was insoluble by heat, but dissolved on the addition of acid. Its appearance was certainly more like urates than phosphates, and I found, on examining it for myself, that the acid only dissolved the

precipitate with heat, but had no action on the cold urine. It was consequently placed under the microscope, and was found to consist almost entirely of globular urates, with high refractive powers, mixed with some crystals of oxalate of lime. Their presence in nearly neutral urine was readily explained by the scanty amount of secretion consequent on the previous severe diarrhœa, but their insolubility by heat was a phenomenon for which I was not prepared. On the following day the urine was more decidedly acid, but there was no precipitate. On boiling, however. a copious precipitate was thrown down, which was immediately dissolved on the addition of a few drops of acid. I have already alluded to the difficulty of explaining the precipitation of phosphates in the presence of acid; when the deposit only occurs after the application of heat the change is still more difficult of explanation, because the urine after being boiled becomes rather more acid than before, as I ascertained distinctly in this case, and the granular precipitate does not dissolve readily with acid after it is allowed to cool.

Formerly we used to be taught that the presence of urates was only due to acidity, of phosphates to alkalescence of urine; but that is a very limited view. It was probably only put forward in this broad form as antagonistic to the common expression of those days, that one or other deposit showed an excess of uric or phosphoric acid, the "lithic-acid diathesis" and the "phosphatic diathesis" of Dr. Prout. It is certain that such a conclusion is very likely to be erroneous, although, perhaps, it contains some element of truth. All that can be said with certainty at present in regard to the uric-acid salts is, that when they are precipitated there must be a certain excess of acidity, and if not an excess of the salt, at least a disproportion between it and the water as ingredients of the urine.

With reference to the presence of phosphates, I conceive that we must assume the same disproportion to exist between the water and the earthy salts whenever the precipitate is abundant, especially when the urine is not decidedly alkaline. It is not an uncommon practice in the present day to maintain a certain degree of alkalescence of the urine both in rheumatism and gout. In such cases it is quite remarkable how the amount of earthy deposit varies while the urine is freely alkaline, but I have long learned by experience that a copious deposit is an indication of depression, and is sure to be followed, if not accompanied, by other symptoms of a corresponding kind. At one time I used immediately to diminish the dose of the alkali, but I have recently observed that, without making any other change in treatment, the addition of a few grains of quinine daily will have the effect of at once diminishing the amount of deposit, and will enable the patient to persevere with such a

dose of alkali as he could not otherwise have taken with impunity. Believing as I do that in both forms of disease it is desirable to neutralise as far as possible excessive acidity, the effect of the quinine is to me a point of great importance in the treatment of these disorders.—*Lancet, July* 22, 1871, *p.* 117.

26.—QUERIES RESPECTING SO-CALLED URÆMIA.

By Dr. H. W. Fuller, Physician to St. George's Hospital.

In acute congestion of the kidneys, epileptiform seizures and other symptoms of so-called uræmia are not unfrequent; and the same holds good respecting chronic Bright's disease. In such cases the amount of urea excreted by the kidneys is far below the normal standard, but nevertheless is often considerable. If the non-excretion of urea is the true and only cause of so-called uræmic symptoms, how is it that coma and convulsions are not present in numberless cases of dwindled granular kidneys, in which the specific gravity of the urine often ranges for years together between 1001 and 1003, and in which therefore the excretion of urea by the kidneys is reduced to a minimum? Again: how is it that persons in whom total suppression of urine occurs, and in whom therefore the excretion of urea through its natural channel is absolutely arrested, not unfrequently go on for a week or ten days without the slightest cerebral disturbance? Such instances are not to be explained by the draining off of the urea by the bowels, for ordinarily the secretion from the bowels is not excessive, or in any respect abnormal; neither is it to be explained by vicarious action of the skin, for very commonly the skin is dry and inactive. Within the last two months I have seen a case with Dr. Playne of Maidenhead, in which a child of fifteen, suffering from scarlatina, had complete suppression of urine for eight days; and yet, with a dry hot skin and only a natural action of the bowels, she not only did not exhibit any symptoms of uræmic coma, or convulsion, but retained full possession of her mental faculties to the last.

Again: if so-called uræmic symptoms were due solely to the presence of urea in the blood, how does it happen that a person ever recovers who has once manifested symptoms of uræmic poisoning, including convulsions and coma? During the fit and subsequent insensibility, the secretions, if not extremely scanty, are certainly not more profuse than before the attack; and presumably, therefore, the quantity of urea in the blood must be as large at the termination of the seizure as it was before the fits commenced. Nevertheless, patients emerge from a long succession of these epileptiform seizures, and

remain for months, or even years, without any farther attack, although the secretions from the skin, the bowels, and the kidneys, continue in precisely the same condition as before the seizure.

Let us, then, look the difficulty in the face, and endeavour to explain it. It is obvious that the non-excretion of urea is in some way connected with the symptoms of so-called uræmic poisoning, inasmuch as they occur only when the secreting power of the kidneys is interfered with; but it is equally obvious that some other factor is necessary to their production, and this is what requires investigation. Until it is clearly understood and taught that so-called uræmic symptoms are not due solely to the presence of urea in the blood, there is little chance of any investigation which is calculated to shed light upon this dark corner of pathology.—*St. George's Hospital Reports, Vol.* v, 1870, *p.* 11.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS—FRACTURES, DISLOCATIONS, &C.

27.—ON ANTISEPTIC SURGERY.

By Joseph Lister, Esq., F.R.S., Regius Professor of Clinical Surgery in the University of Edinburgh.

[Professor Tyndall in one of his experiments showed that a mass of cotton-wool has the power of filtering the air passed through it of its suspended particles. He blew against a beam of condensed light entering a dark room, with a pair of bellows having a mass of cotton-wool tied over the nozzle; the result being, that the beam, elsewhere white from illuminated dust, became perfectly black at the part on which the current was directed through the cotton filter; hence the idea has naturally suggested itself that cotton-wool might be used with advantage as an antiseptic dressing.]

It would be useless to apply ordinary cotton without special precautions, for, according to the germ-theory, putrefactive particles must exist among the fibres and lie scattered over the wool. But if the cotton were impregnated with some volatile material capable of destroying the vitality of the septic organisms, and then placed upon the wound after washing it with a lotion containing the same substance in solution, the result ought to be, supposing the theory true, that, after the volatile antiseptic had become dissipated by diffusion from the dressing and from the wound, the cotton-wool, though destitute of any chemically antiseptic properties, should effectually prevent, by its filtering property, the access of any putrefactive agents, and keep the wound sweet, while in itself a perfectly bland and unstimulating application. Accordingly I prepared four samples of cotton-wool by diffusing through each one of the following substances—chlorine gas, sulphurous acid gas, carbolic acid vapour, and the vapour of benzine—four materials very dissimilar in chemical properties, but having a common hostility to low forms of life. Chlorine, sulphurous acid, and carbolic acid, are well known to have such a property; and,

knowing that benzine is used by the entomologist for killing insects, and having ascertained by experiment the potency of its vapour for the destruction of pediculi, I thought it probable that it would also answer our purpose. I then dressed with these four kinds of prepared cotton-wool various suppurating sores, excoriations, and contused wounds, after washing the surface with the corresponding lotion, or in the case of benzine, with the undiluted material. The results in every instance corresponded exactly with theory. After about twenty-four hours' exposure at the temperature of the body, the cotton-wool was found to have lost the odour of the antiseptic, yet the blood, serum, or pus, as the case might be, remained perfectly sweet for an indefinite period, while healing advanced in the satisfactory manner that might be anticipated from the absence of all irritating quality in the dressings. There was, however, one circumstance, highly instructive in itself, which interfered sadly with the utility of this application; namely, that, if the discharge happened to be sufficiently copious to soak through the cotton-wool and appear at its external surface, putrefaction occurred throughout the entire mass of the moistened part down to the wound, even within the first twenty-four hours after the dressing, if the fluid were sufficiently copious to penetrate within that period. It is only when dry that cotton-wool can arrest the progress of microscopic organisms, which have ample room to develope among its meshes when filled with a putrescible liquid.

And now, allow me to endeavour to bring home to you a little more closely the inference that is to be drawn from these facts. But, first, let me describe in detail the manner in which the dressing with carbolated cotton-wool was practised. The cotton-wool having been impregnated with about a two-hundreth part of its weight of the acid in the form of vapour, the surface of a granulating sore or abrasion was washed, together with a portion of the surrounding skin, with a solution of the acid in about forty parts of water. A piece of oiled silk of the size of the sore was then applied, to prevent the dressings from sticking through dryness. Over this was placed a piece of folded linen rag, rather larger than the oiled silk, and impregnated with the carbolic acid vapour in the same manner as the cotton-wool; the object of the rag being to absorb the discharge and prevent it from trickling down, as it was otherwise apt to do, below the slightly absorbent cotton, involving its early appearance at the surface and consequent spread of putrefaction to the wound. Lastly, a well overlapping mass of the carbolised cotton-wool was securely fixed by a bandage. The result, as before stated, was that, though all chemical antiseptic virtue left the dressing within a day or two, putrefac-

tion was excluded by the cotton-wool for any length of time, provided the discharge did not penetrate to the exterior of the mass. Consider, now, the circumstances of the serum or pus that oozed from beneath the edges of the oiled silk into the folded rag—let us suppose a week after the application of the dressing, when all traces of the volatile antiseptic had certainly disappeared. Here was a highly putrescible liquid, not subjected to boiling, as in the flask experiment, or acted on by any chemical agent whatever, yet remaining free from putrefaction in a rag moistened with it at the temperature of the human body, simply because it was covered over with pure dry cotton-wool. How, then, did this cotton-wool exclude the causes of putrefaction in the atmosphere? It certainly did not keep out any of the atmospheric gases. The same cause that led to the escape of the volatile antiseptic necessarily occasioned a perpetual intermingling between the external air and that between the meshes of the fabric, as any one acquainted with Graham's beautiful researches into the laws of gaseous diffusion must at once admit. The only constituent of the atmosphere which the cotton-wool could possibly exclude is its dust; and this we know, from Tyndall's experiment, it did exclude. Here, then, we have another inevitable inference from fact, another truth, and that in itself all-sufficient, with reference to the antiseptic system of treatment; the truth, namely, that pus, blood, and the dead tissues in contused wounds do not putrefy through the influence of the atmospheric gases, but through the operation of particles of dust, which may be permanently deprived of septic energy by the vapour of an agent like carbolic acid. I do not ask you to believe that the septic particles are organisms. That they are self-propagating, like living beings, and that their energy is extinguished by precisely the same agencies as extinguish vitality, such as heat and the various chemical substances to which I have referred, is certain, and is of the utmost practical importance. But if any one, in spite of these facts, and in spite of the strong analogy of the yeast-plant, and the various kinds of fungi which we term mould, prefer to believe that the septic particles are not alive, and to regard the vibrios invariably present in putrefying pus or sloughs as mere accidental concomitants of putrefaction, or the results, not the causes, of the change, with such an one I, as a practical surgeon, do not wish to quarrel. Nor do I enter upon the question whether spontaneous generation can take place at the present day upon the surface of our globe. To do this, would be to engage in doubtful disputations which I promised to avoid.

But I do venture earnestly to beg of all of you who are engaged in surgical practice, that you will give these simple facts

your careful consideration; and if you think the interpretation I have given a sound one, do not let any statements, whether in books or in journals, shake your belief in the truth that putrefaction, under atmospheric influence, as it occurs in surgical practice, is due to particles of dust ever present in the atmosphere that surrounds our patients, and endowed with wonderful chemical energy and power of self-propagation, yet happily readily deprived of energy by various agents which may be employed for the purpose without inflicting serious injury upon the human tissues. With this as your guiding principle, you will find yourselves successful with the antiseptic system of treatment; but without it, whatever theory you adopt, you will ever be walking in the dark, and therefore ever liable to stumble.

And now I proceed to the second division of my subject—the exhibition of our principal means and methods of treatment. For preventing the access of putrefactive fermentation, the agent which we now commonly use is what we have termed the antiseptic gauze, of which these are samples—being a loose cotton fabric, the fibres of which are impregnated with carbolic acid securely lodged in insoluble resin, which holds the carbolic acid with remarkable tenacity, while at the same time a little paraffin is added to prevent the adhesiveness which the mixture of carbolic acid and resin would otherwise possess. The interstices betwen the fibres are kept free from these ingredients, so that the fabric, being porous, may be fitted for absorbing discharges. The carbolic acid is in considerable quantity in the gauze; but it is held so tenaciously by the resin that, on the one hand, when first applied, it is unirritating to the human skin, and, on the other hand, unless discharge be very copious, it will retain its virtues for upwards of a week at the temperature of the human body. Now supposing I were going to use this gauze for dressing any case in which a copious discharge was expected—as, for example, a large psoas abscess immediately after it had been opened—I should take a considerable quantity of the gauze (about as much as one can conveniently hold between the extended hands) and fold it three times so as to make it eight layers. But there would be no use in my having the folded gauze of this extent, if I did not adopt some means for compelling the discharge to pass throughout the length and breadth of the dressing; and for this purpose some impermeable tissue must be interposed between it and the external air. That which we have found the most convenient is a cheap and light form of mackintosh, termed "hat-lining" by the india-rubber dealers. I cut a piece of this, nearly as large as the folded gauze, and then place it beneath the layer that is intended to be outward. The discharge then coming from the

wound, situated opposite the middle of the gauze, instead of passing directly outwards through it, is compelled to traverse all the extent of the antiseptic dressing: and in that way, by using a sufficiently large piece, and with this arrangement of the mackintosh, you may be perfectly certain that, if you leave no putrefactive mischief in a wound or abscess, none will enter it, however profuse the dischage may be during the first twenty-four hours. That is one very important point gained. As the discharge diminishes, the intervals between the dressings are extended; and when it amounts to only a minim or two in twenty-four hours, the application may be left undisturbed for a week. The gauze is also extremely convenient in the form of bandage—an antiseptic bandage—which is put on to hold the main dressing in position; and instead of being a nidus for putrefaction, as a cotton-bandage would be, it increases at every turn the antiseptic efficacy of the dressing. Besides this, the bandage having a degree of stickiness, its turns do not tend to slip as those of a cotton-bandage do, which is an additional advantage.

Such, then, are the means by which, in ordinary cases, we ensure that putrefactive fermentation does not extend from without into the wound or abscess. Of course it would be of no use to apply such an external dressing if putrefactive particles in an active state were left within a wound. If a wound be presented for treatment, having been inflicted by another than the surgeon, some dust is sure to have been introduced; and we must first destroy its septic energy by washing the raw surface thoroughly with some liquid trustworthy for the purpose, such as chlorine-water, or sulphurous acid lotion, or a strong solution of carbolic acid, or of chloride of aluminium, for there are various preparations which may be used with efficiency. But when the surgeon operates on a previously unbroken integument, he has the opportunity of preventing the septic particles from entering in an active state at all, by operating in an antiseptic atmosphere. This is readily provided for in small operations by using a watery solution of carbolic acid with Richardson's apparatus for local anæsthesia. For making the spray more fine, I have found it convenient to have the lower end of the water-tube almost entirely stopped up, leaving only very minute apertures. The result is, as you see, an exceedingly satisfactory spray. For any small operation this answers the purpose perfectly well, provided always that you take the precaution of having the liquid filtered through a cambric handkerchief or some similar fabric, in order to exclude the grosser particles of dust, which otherwise would have the effect of blocking up the fine orifice at the nozzle of the apparatus—an occurrence which, under some circumstances, might be disastrous in its effect.

We have lately found that the strength of the solution employed for producing the spray may be considerably reduced. We have ascertained that it may be used as weak as one part of carbolic acid to a hundred parts of water; and that a spray made with such a lotion is thoroughly trustworthy as an antiseptic atmosphere.

The reduction of the strength of the spray is a matter of great importance. In the first place, it is a great comfort to the surgeon, as I can testify from experience. When we used a solution as strong as one part of carbolic acid to forty parts of water, my hands were constantly in a rough and uncomfortable state; but when the proportion is reduced to one to a hundred for the production of the spray, the hands experience no inconvenience whatever, and one can even breathe with comfort in such an atmosphere.

In the second place, it is equally advantageous for the patient, because the weaker the antiseptic application, of whatever sort it is, the less irritation do we occasion to the tissues of the part treated with it. The antiseptic is always injurious in its own action; a necessary evil, incurred to attain a greater good. To suppose that it is useful by its own operation in some specific manner unknown to us, is an entire mistake. I know that, not only from theory, but as a matter of experience. At one time, I used the undiluted acid; and, in doing this, I could not avoid producing not merely irritation, but a certain amount of sloughing. Then I used a strong solution of carbolic acid in oil; then a rather strong solution in water; then a weaker watery lotion; and now we employ a solution as weak as that which I have described—one part of carbolic acid in a hundred of water—and that applied only in the form of spray, avoiding absolute drenching of the tissues at all, and avoiding also the injection of the wound by a syringe, as we used to do after the operation was completed, in order to destroy the organisms introduced; and, in direct proportion to the weakness of the solution used and to the smallness of its opportunity of acting on the tissues of the part, is the satisfactoriness of the results obtained, provided that the essential object of avoiding putrefaction is secured.

And now, supposing that I were single-handed, about to change the dressing in the case to which I have alluded—a large psoas abscess—the spray is of extreme value. I wish that the spray shall play upon the surface of the body, in the angle between the dressing and the skin, as I lift the gauze. It would be very inconvenient if it were necessary for this purpose always to have an assistant to work the spray; but, by a little management, the spray can be worked perfectly well, as you see, by the surgeon himself. [This is done by placing the

bottle of Richardson's apparatus against the ball of the thumb, and holding the India-rubber bulb to be compressed between the opposite side of the bottle and the fingers of the same hand.] Supposing this were the site of the incision in a case of psoas abscess, as long as I choose I can perfectly protect it with the antiseptic atmosphere, and then put on what we have called, for the sake of distinction, a "guard"—a piece of rag dipped in the one to one hundred watery solution of carbolic acid, after which the spray can be removed with security; the surrounding parts having then been cleansed from any discharge there may be, the spray is once more made to play on the part during the exposure of the wound until the permanent antiseptic dressing is re-applied.

But though such a spray-producer is perfectly efficacious for a small operation, it does not make a cloud of sufficient volume for a large one, such as an amputation of the thigh or at the hip-joint. Therefore, with the object of securing the same result in such cases, I have had this apparatus prepared, which, I confess, is in a cumbrous and heavy form; but I hope it will be improved in that respect before long. Meanwhile, it is much better than nothing. Let me say a word or two, in the first place, as to the principle on which it is constructed. It appears that the best kind of spray which can be produced, is that which is formed on the principle of the atmospheric odorator, by having one tube set at right angles to another, the air-tube being larger than the water-tube, and the opening of the water-tube being exactly opposite the middle of the orifice of the air-tube. This makes the finest and best of all sprays. But, with a heavy apparatus like this, it would never do to have to move it about along with the nozzle, as is absolutely necessary in the instruments of ordinary construction on this principle. We must have tubes to convey the air and the water to a considerable distance; and this is very easily done by not merely having the liquid ejected by the force of the air blown over the orifice of the water-tube, but by having it driven through the tube by the force of the same pump that propels the air, the quantity of the water being regulated by a stop-cock. Then it was necessary to provide some ready means of clearing the fine end of the water-tube, in case of its obstruction by particles of dust. This is done by having the water-tube straight for a short distance from the nozzle, and then bent at a right angle, with a little milled cap to screw on at the angle, so that, in case of obstruction, the cap is screwed off, and the orifice of the water-tube is cleared at once with a needle or a bit of fine wire. I have used this apparatus in various operations of late, among which I may mention my two last amputations, one in the thigh, the other in the arm, in both cases using nothing

stronger than the one to a hundred solution for the spray, and the same for the sponges; exeept only, what I believe to be a wise precaution, that, when a sponge has become soaked with blood, it should be washed first with pure water, then dipped for a moment in a strong solution (one to forty), and then squeezed out of a solution of one to a hundred to give it the neeessary blandness; and in both these cases putrefaction was entirely avoided. [The apparatus exhibited had two nozzles, attaehed to independent caoutchouc tubes, furnishing large clouds of spray, that could be directed, if necessary, to opposite sides of the part operated on. Two of Richardson's spray-produeers, worked by two assistants, will answer the same purpose, though less efficiently.]

The antiseptie catgut-ligature is used for securing the arteries while the spray still plays over the wound. It is absolutely neeessary that it should be properly prepared. I must not enter into the method of preparation, further than to say that catgut undergoes a remarkable change in its physical eonstitution when steeped for a long time in an emulsion of water and oil, so that it becomes quite transparent, and no longer liable to beeome soft and slippery when placed in water or in a watery diseharge. But for this eireumstanee, the animal ligature would be an impossibility; but, if you use it properly prepared, you will, I believe, see good reason to be satisfied with it. That whieh I now show is extremely fine, mueh finer than any silk commonly employed; and yet with a piece like this I should not hesitate to tie the femoral artery in a stump. If you ehoose to use it thicker for a large vessel, you can do so. It is conveniently carried on a little winder, in a capsule appended to a caustic case. The catgut, as tied in the ordinary reef-knot with the ends cut short, seems to me to be a perfect hæmostatic. It has all the simplicity and universal applicability of the ligature, with, at the same time, the virtual absenee of any foreign body from the wound. If putrefaetion be avoided, it is rapidly absorbed, and you may reckon as eertainly on the absence of any interferenee with primary union on the part of sueh ligatures, as if there were no ligatures at all. Should putrefaction oceur, I was at first uneasy lest the prepared catgut might soften and permit hemorrhage. I was, therefore, at the pains to test some of the prepared catgut in the following manner. I tied some pieees of it at intervals round a cylinder of India-rubber, so as to pinch the India-rubber to a considerable degree of constrietion, and then introduced it into putrid serum of blood, and kept it for a week at a temperature of about 90 deg. At the end of this period, the India-rubber was still constrieted, shewing that the catgut had retained its hold in the putrid liquid, in spite of the constant strain of the elastic material upon

the knots. No doubt, in such parts of a wound as actually putrefy, the little bits of catgut must come away like shreds or sloughs of cellular tissue; but I am bound to add that this is only a matter of presumption: for, although I have used nothing but this ligature for securing vessels in wounds for more than two years, excepting torsion, which I comparatively rarely resort to, and though in certain classes of cases putrefaction cannot be avoided, in no instance have I seen the catgut knot come away, nor have I ever known secondary hemorrhage or abscess caused by its use.

I have spoken of the injury that the stimulating carbolic acid lotion inflicts on the tissues by irritation. The great disadvantage of this is, that it causes an unusually large flow of serum during the first twenty-four hours or more; and you must provide a special exit for the serum, else you will have inconvenience from tension, which will lead to suppuration, though not of the putrefactive kind. For the purpose of guarding against this, I introduce, at the most dependent part of the wound, a strip of lint steeped in a solution of carbolic acid in about ten parts of olive oil, to serve as a "drain." This is drawn out under the spray in twenty-four or forty-eight hours. If you drew it out without providing an antiseptic atmosphere, you would certainly have putrefaction. In some cases, a fine drainage-tube is convenient for this purpose, if well steeped in solution of carbolic acid. For, as India-rubber happily absorbs carbolic acid, the drainage-tube is antiseptic when introduced.

There is yet one other point to which I must allude, which is that carbolic acid interferes with the cicatrisation of a wound, if it act directly on it. This agent operates with special energy on the epidermis. Sometimes this is a convenience. For example, if we dip the forefinger into a carbolic acid lotion, and hold it there for a second or two, we may be certain that the epidermis is so imbued with the carbolic acid, that it is for the time antiseptic, and therefore may be introduced into the cavity of an abscess or any other part which we wish to explore; and very valuable an antiseptic forefinger often is in that way. But this action of the acid on the epidermis makes it interfere with cicatrisation; and even the gauze, though generally perfectly free from irritating influence upon the sound skin or an old scar, will frequently, if applied directly to a wound, entirely arrest new epidermic formation, and sometimes excoriate a tender young cicatrix. Something, therefore, must be interposed to protect the wound from this effect of the antiseptic. What we have generally used hitherto for this purpose is what we have called the "oiled silk protective," consisting of oiled silk varnished with copal varnish, which makes it much less permeable to the carbolic acid. But unfortunately, this is not

a perfect protective. It acts admirably until it becomes moistened; but afterwards the water that penetrates the substance conveys the carbolic acid in. I have striven in various ways to get something perfect in that way; and I have lately been engaged in a manner which, though not yet completely successful, may be mentioned on account of its interest otherwise. Some time since, I tried the effect of an oil-paint on oiled silk, in the hope that the particles of pigment, closely packed, might serve considerably to intercept the carbolic acid, though the oily material that cements the particles is permeable to it. The result was such as I had hoped, except that the material proved too stiff for convenient use. A few weeks since, however, I happened to be going through an India-rubber factory, and there I saw, among other things, the process of mixing various pigments with caoutchouc; and it occurred to me, might not India-rubber, blended with some pigment, answer as a protective? The India-rubber is permeable to the carbolic acid; but with the pigment it might not be so. I first tried a coloured rubber that had been vulcanised, and then came out a most curious and interesting circumstance. The sulphur in the vulcanised India-rubber acting chemically on the discharge, the result was a stench like rotten eggs, presenting an excellent example of decomposition without putrefaction; for there was no putrefactive fermentation—no spread of the decomposition into the interior of the wound or abscess. It was limited to the exterior, and was simply the result of the chemical action of the nascent sulphur upon the discharges. And if, under such circumstances, we resumed the oiled silk protective, we again had perfect absence of unpleasant smell.

The necessity for avoiding any sulphur in the material was a great cause of embarrassment; for, as a general rule, the admixture of any foreign ingredient with caoutchouc causes a most inconvenient softness and adhesiveness of the product—evils which vulcanising completely corrects. Magnesia forms an exception to this rule, producing with the pure rubber a very satisfactory substance as regards its physical properties. But then we found that, in the case of a sensitive skin, this magnesia caoutchouc produced intolerable itching and redness, for a reason which I do not quite understand. At length it occurred to me that perhaps shell-lac, which seems quite unirritating, might be mixed with the caoutchouc; and that this might answer the purpose. For though shell-lac, when once mixed with carbolic acid, holds it very tenaciously, as is seen in the lac plaster with which some of you are familiar, yet the acid does not readily penetrate into unmixed lac. When I suggested this to the managers of the India-rubber works, they told me that they had previously ascertained that shell-lac

could be perfectly blended with caoutchouc; the product being the beautiful article you now see, sufficiently tough, yet pliant, transparent, and with no unpleasant odour, and, as I ascertained by experiment, practically impermeable to carbolic acid. Here, then, I thought I had attained the object at which I had been aiming for years; and already we were getting results of a kind we had never got before: we had reached more nearly than ever before the conditions which we know must occur subcutaneously. I had never witnessed the healing of ulcers proceed so rapidly as I have seen it under this protective, covered with overlapping gauze; but, to my extreme chagrin, I have learnt within the last few days that, in two patients with very sensitive skins, even this material produces a trifling irritation. Still I cannot but believe that we are on the verge of getting what we want in a protective—viz., a tissue perfectly bland and unstimulating in its own substance, and also quite impermeable to the antiseptic.—*British Medical Journal, Aug.* 26, 1871, *p.* 226.

28.—ON ANKYLOSIS.

By Bernard E. Brodhurst, Esq., Orthopædic Surgeon to St. George's Hospital.

[On determining on the treatment to be adopted in a case of ankylosis of a joint, we consider less the position of the limb than the character of the adhesions. Soft or fibrous adhesions we call false ankylosis; and hard or bony adhesions we call true ankylosis.]

False, fibrous, or partial ankylosis, then, is occasioned by the deposition of lymph within or around a joint, through which adhesions are formed, which interfere with motion; while true, complete, or bony ankylosis, or synostosis, is that condition in which the soft structures of the joint have been destroyed, and bony union has taken place between the adjacent and bony surfaces.

False ankylosis may be *intra-capsular;* which signifies a junction through membranous bands or fibrous tissue of the contiguous surfaces of the bones which enter into the articulation; or it may be *extra-capsular*, in which case lymph is deposited external to the capsule, either in the shape of membranous bands, or there may be produced thickening of the capsule, or of a part of the capsular and ligamentory tissues around the joint.

In many instances, however, the structures both within and beyond the capsule become inflamed and thickened; lymph is poured out into the cellular tissue and about the sheaths of the

tendons and muscles, and the various structures become more or less matted together, whether in an extended or a flexed position; and lymph is also deposited upon the synovial membrane, through which adhesions form and motion is hindered. In some cases the adhesions which have formed are slender, but they may, notwithstanding, prevent useful motion; or again, they may be more extensive, and yet admit of treatment to restore motion. Inflammation may extend from without inwards, or the reverse may take place. Thus, of the first description, we have inflammation arising from mechanical injuries, phlegmonous erysipelas, and burns; while of the latter, acute synovitis, as from over-exercise, and strumous, rheumatic, syphilitic, and gonorrhœal inflammations, are the chief forms.

Each of these forms of inflammation presents characters peculiar to itself in the ankylosis which results; so that it may for the most part be predicated what form of inflammation gave rise to the joint affection. It is of importance, in the treatment of ankylosis, to distinguish the various results of these different forms of inflammation—to recognise in the result the amount of force that may be necessary to overcome the rigidity of the joint; for as the sensation which is communicated to the hand in bony ankylosis is altogether different from that which is presented by fibrous ankylosis, so is there scarcely less difference between the firm fixed condition produced by gonorrhœal rheumatism, and the soft adhesions which result from scrofulous inflammation.

The *treatment* of fibrous ankylosis may be divided into, 1st, gradual extension with or without tenotomy; and, 2nd, immediate flexion of the limb with or without tenotomy, and subsequent gradual extension.

In all cases of partial ankylosis there exists some muscular rigidity; in some cases, also, cicatrices are found, resulting from loss of substance. Where adhesions are recent, contraction of a limb may probably be overcome by continued extension—such extension, namely, as is made by means of a well-adjusted instrument for the purpose; but, except in cases of recent adhesions, it is generally necessary to commence the treatment by dividing the tendons of rigid muscles, and by dividing cicatrices subcutaneously. It is better to proceed at once to these subcutaneous sections, rather than to prolong the treatment by extension unnecessarily; for, unless the adhesions are recent, simple extension is seldom of itself, and uncombined with subcutaneous section, sufficient to remove the contracted condition of a limb. It is important to remember this principle of treatment; for partial displacement of the articular surfaces is easily induced by continued extension of the limb, if

the tendons have not been previously divided. Indeed, it is not uncommon to see this displacement take place at the knee when extension is long continued, and where the tendons have not been divided. Whenever, therefore, it is desired to remove contraction, it is the rule first to divide the tendons of rigid muscles, and to divide cicatrices subcutaneously, and subsequently to proceed gradually to extend the limb.

But if such be the law of treatment where the articular surfaces occupy their normal positions, it is even more to be insisted on when any displacement has taken place. Extension should then, without fail, be preceded by the subcutaneous section of such tendons, fasciæ, and cicatrices, as might interfere with the readjustment of the articular surfaces.

These obstacles to extension, then, having been removed, a well-adapted instrument is to be applied to the limb, and extension is to be made slowly. The instrument should support the limb efficiently; and it should always, in the first instance, be applied at that angle, whatever it may be, at which the limb was held before the subcutaneous sections were made. So soon, then, as the punctures have healed, extension may commence, and be carried on gradually, without producing pain, and without occasioning displacement.

Numberless cases, however, exist in which the means above mentioned are useless to restore to the limb either the normal position of its parts or to restore motion; cases, for instance, in which the adhesions are so firm that they do not yield to gradual extension. The pressure produced by continued extension may occasion destruction of the integument, or it may induce displacements, partial or complete, of the articular surfaces; but the adhesions, whether intra- or extra-capsular, will not yield to such force. Injury alone, but no useful result, can accrue from gradual extension in these cases. Before chloroform was introduced, these were among the *opprobia* of surgery. Then, gradual extension of such limbs was continued for months and years, without any advantage being derived.

I know an instance where an advocate of the gradual extension system, and gradual extension only, continued to apply his means to a case of ankylosis of the knee for the space of fourteen years. The extending apparatus was removed by another surgeon, and the knee was excised, because bony ankylosis had taken place at an inconvenient angle. Probably, if left to himself, the first operator might have continued his efforts at extension, in the firm hope of ultimate success, after another period of fourteen years.

It is necessary, therefore, before proceeding to the treatment of a case of this kind, to form a correct diagnosis—to deter-

mine whether complete ankylosis has taken place, or whether the adhesions are fibrous; and if fibrous, whether they will or will not yield to gradual extension. If these several points cannot be otherwise determined, chloroform should be fully administered; so that, when muscular relaxation has been obtained, both the character of the adhesions and the amount of motion may be ascertained.

When bony union has taken place, a sense of solidity and continuity of structure is communicated to the hands on grasping the limb above and below the articulation; but when fibrous adhesions have formed, either slight motion may be felt at the articulation, or at least a sense of elasticity is communicated, on endeavouring to flex the limb. And if the adhesions are of such a character, so firm and unyielding, that the normal position of the limb can only be gained by force suddenly applied to rupture the adhesions, the force should be so applied that it is used mainly, if not entirely, in the flexion of the limb.

It was only when tenotomy and anæsthesia were combined that the operation of forcible flexion could be looked upon as free from danger. I have performed 267 operations of this nature, and I have never known any *contretemps* whatever—neither fracture, nor dislocation, nor pyæmia, nor inflammation.

Any tendons which are rigid should first be divided; and the punctures having healed, and chloroform having been fully administered, the limb to be operated on should be so firmly fixed that all motion is prevented, except that which the operator is about to impart to the limb. Thus, for instance, if the hip-joint is to be operated on, the pelvis must be fixed; if the knee, the thigh must be securely held; and so on. When the limbs are thus firmly secured, the adhesions are to be instantaneously ruptured by force applied in the direction of the flexion. I say that the adhesions are instantaneously ruptured, when the patient is properly prepared, and the force is rightly adjusted. The limb is then to be bandaged; and especially the affected joint is to be firmly bandaged, and confined either in a gutta percha splint or on a flexible splint. This bandage should on no account be removed until tenderness has ceased. The articulation should be firmly encased in the bandage; and should this become loose, another should be placed tightly over it, and be allowed to remain so long as any tenderness continues. It is rare, indeed, that the bandage requires to be moistened.

I know of no danger whatever from the use of force thus applied. Indeed, when the influence of the muscles is perfectly removed, the adhesions themselves usually offer very little

resistance; and if the power to be applied is sufficient for the purpose, the result is instantaneous. In a small number of instances, the hand alone is insufficient to rupture the adhesions readily; and in these I make use of an instrument to flex the limb. Not only is there no danger connected with this operation, but with moderate care it would seem to be impossible to set up unhealthy action. It is sometimes said that in these operations fracture is not uncommon, and that inflammation is not unfrequently excited. Let it be sufficient for me to say, that I have never seen a fracture produced, nor have I known inflammation to occur, nor any other ill whatever to follow an operation of this nature; and that when disaster ensues, it is from abuse of the operation. Doubtless this operation is capable of abuse, just as is any other operation; but when it is performed, as I have endeavoured to describe it, I do not know an operation more successful than this in the whole range of surgery, nor one more free from danger.

When the joint retains its normal external form, the adhesions are easily broken down by the hand, after the limb has been properly placed in position and the full effect of chloroform has been obtained. There was lately a case under my care in the hospital, where the patient, having suffered from rheumatic inflammation, was admitted with partial ankylosis of the knee and of the ankle. The tendo Achillis was, in the first instance, divided; and after the puncture had healed, the adhesions at the knee joint were ruptured by flexing the foot upon the leg. On a subsequent occasion the hamstrings were divided subcutaneously; and, the punctures having healed, the adhesions at the knee-joint were ruptured by flexing the leg upon the thigh. This patient walked well when she left the hospital, and without lameness; and the movements at the knee and ankle-joints of the affected limb were as free as those of the sound limb.

It is a point to remember, that after dividing the tendons, and before the punctures have healed, the adhesions should not be ruptured; or they should be ruptured only with great care, lest the puncture should be extended into a rent. This extension of the puncture is much easier to effect than might be supposed; and it is, therefore, safer to allow the punctures to heal before any force is employed.

When the position of the limb is perfectly restored, then passive motion should commence. At first it may be necessary to administer chloroform, for motion is painful; but as motion increases, passive movements excite less pain.

True Ankylosis.—Complete, bony, or true ankylosis, is rare. When it has taken place, a sensation of solidity is communicated on grasping the limb above and below the articulation,

such as can only be occasioned by continuity of bony structure. In fibrous ankylosis this sensation is never experienced. Fibrous ankylosis may, however, allow of so little motion, that with rigid muscles it may be inappreciable until chloroform has been inhaled. Therefore, as motion is thus masked, and as bony ankylosis is rare, it is safer not to express an opinion in favour of bony ankylosis until chloroform has been exhibited. When the patient is under the influeuce of chloroform, no doubt can exist as to the nature of the adhesions.

Bony ankylosis is the result of inflammation and suppuration within the joint, together with the destruction of the articular cartilages. Ankylosis may then result between the exposed surfaces of the bones, if the inflammation be of a reparative nature, causing the deposition of new bone. When destructive inflammation ensues, causing necrosis of the epiphyses, bony union is impossible. Diseased action must cease before repair can commence; and bony union is repair. This repair, however, may be of such a character as to be useless, and even worse—it may be detrimental. Such cases then admit of treatment.

Ankylosis is occasionally observed as a congenital affection: the articular apparatus is then entirely absent. In these cases an inconvenient angle is never found, and the reparative process is as complete as nature can make it.

In bony ankylosis the articular extremities are either bound together in the course of the ligaments, or the bones are united in their entire thickness; two becoming fused into one. It has occurred to me once to see a living person without a single movable articulation—every joint was ankylosed.

Bony ankylosis is, however, rare; but union may take place at such an angle as to be in the highest degree inconvenient. Under these circumstances, this repair, which is intended to be permanent and useful, may fairly be made the subject of surgical interference.

Treatment.—There are four operations which may, under certain circumstances, be done to restore motion or to improve the position of the limb—viz., 1st, to remove a wedge of bone; 2nd, to break through the ankylosis, after drilling through the new bony formation; 3rd, to make a false joint; 4th, to divide the bone subcutaneously, and restore the position of the limb.

[Mr. Brodhurst then gives an account of these operations as performed by Dr. Barton, of Philadelphia; Professor Brainard, of Chicago; and himself, which we advise the reader to peruse in these valuable reports of St. George's Hospital.]—*St. George's Hospital Reports, Vol.* v. 1871, *p.* 149.

29.—ON A NEW APPARATUS FOR THE TREATMENT OF FRACTURES.

By Dr. ANTHONY H. CORLEY, Surgeon to Jervis-street Hospital, and Lecturer, Carmichael School of Medicine, Dublin.

[In describing and bringing under the notice of the profession a new way of using plaster of Paris in the treatment of fractures, Dr. Corley lays no claim to originality. He has found the plan both convenient and efficient, and it appears to us worthy of trial.]

We are accustomed to summarize the treatment of fractures of the extremities in the four words: *Extension*, *Counter-extension*, *Co-aptation*, and *Retention*. Every practical surgeon knows and appreciates the difficulty of fulfilling the last of these indications. The variety and multiplicity of splints, bandages, and other means of retaining the fractured ends in apposition, sufficiently testify to the importance of the object to be secured and the attention bestowed on it, In Jervis-street Hospital, where patients with fractured limbs are admitted almost daily, it is a matter of necessity to practise some method of treatment which will do full justice to the sufferer, whilst involving the least trouble and loss of time to the surgeon. Since the introduction of starch and plaster of Paris bandages, the benefits conferred by these have been freely taken advantage of, but their use in the ordinary way is attended with very serious objections. The first of these is the trouble and time spent in their application; and the next, a still stronger argument against them, is the difficulty of *taking them off*. Even with the facilities afforded by Seutin's Pliers, and other resources, the removal of the rigid apparatus is a work of much difficulty. This makes the surgeon unwilling to apply it until the fracture is so far consolidated as to no longer require frequent inspection. In the institution named, neither starch nor gypsum is ever used until about three weeks from the date of the accident, and in most cases during that entire period the patient is confined to one position. Starch bandages, &c., have the great disadvantage of requiring some days to harden, and unless the fractured bone be tolerably well united, or the most constant attention be paid during the time of drying, the ultimate removal of the apparatus may disclose a deformity which is for the remainder of the patient's life a practical illustration of careless surgery. Gypsum hardens at once, but has the same defect in difficulty of removal, and cannot therefore be applied until the period before referred to. The irksomeness, and even danger, attendant on confinement to a constrained position can scarcely be over-rated, especially

in old people, and the chief advantages of the apparatus which I am about to describe are, that it can be used at a much earlier period, and that when applied the patient may lie in almost any position.

In the appendix to the *Army Medical Report* for 1869 (just published), it is described with an illustration, by Staff-assistant Surgeon Moffitt, who states that it is used by the Bavarian Ambulance Corps. Two pieces of flannel, suited to the length of the limb, are cut sufficiently wide to overlap slightly in front. When so prepared, they resemble the leg of a stocking cut vertically. One is now laid over the other, and they are stitched together from top to bottom, down the mesial line, like two sheets of note paper stitched at the fold. They must now be spread out under the injured limb, so that the line of stitching corresponds to the back of the calf. The two inner leaves, so to speak, are now brought together over the shin, and fastened by long pins, the heads of which are bent. The leg being held firmly, an assistant mixes the plaster with about an equal bulk of water, and rapidly applies it, partly with a spoon and partly pouring over the outer surface of flannel *covering the limb.* The two portions of the second layer are then quickly brought over, so as to meet, and the inequalities in the distribution of the plaster are removed before it hardens, by smoothing with the hand. In about three minutes the gypsum sets, and the limb is encased in a strong, rigid covering, which gives uniform pressure and support to every part. The edges of the flannel in front can now be trimmed, and the pins withdrawn from the inner layer, by seizing their bent heads, A couple of straps, or a few turns of a roller, make all secure. In order to take the apparatus off, it is only necessary to remove the straps and separate the edges of the flannel, when the two sides will fall asunder, the line of stitching behind acting as a hinge.

The application takes less than ten minutes, the removal about two. Thus, from day to day if necessary, the limb can be inspected, and the splints (for they are no less) re-applied. In cases of compound fracture, an opening suitable to the wound may easily be made. In most cases it is desirable to make a number of perforations with a gimlet, so as to prevent unnecessary heat. The following cases, treated within the last month, or still under treatment, illustrate its efficiency :—

Case 1.—Simple fracture of fibula, immediately above the malleolus. In this case the patient, a restless, uneasy, middle-aged man, could not be induced to keep his leg in a box or splints, but when the plaster and flannel were applied he expressed the greatest satisfaction. As he was a tailor by trade, and not requiring to stand or walk, I allowed him to

leave the hospital at the end of a fortnight, with instructions to wear the plaster for a month.

Case 2.—Fracture of fibula, about its centre, in a young boy. Apparatus applied at the end of a week, and in a few days he was able to walk with crutches.

Case 3.—Oblique fracture of both bones of leg in lower third, much shortening, rotatory deformity, ecchymosis and swelling. This last sign was so marked that it was not deemed judicious to apply the Bavarian apparatus until the fifteenth day. The patient, a man of about forty years of age, experienced much relief, could keep the limb in any position, and move it so freely that he complained of the weight of the apparatus interfering with his movements.

Csse 4.—Fracture of both bones of leg immediately above ankle-joint, with much twisting inwards of the foot. For the same reason as in the last case, the plaster was not applied until the twelfth day. While the plaster was setting the foot was kept turned in the requisite direction, and in hardening the correct position was retained. The patient, a young girl, felt quite comfortable in a few minutes.

Two of the above cases, with others since admitted, are still under treatment. In conclusion, I believe it will be found that the thanks of the profession are due to Dr. Moffitt for introducing such a valuable adjunct to surgical appliances.—*Dublin Quarterly Journal, Aug.* 1871, *p.* 62.

30.—ON THE TREATMENT OF HIP-JOINT DISEASE.

By Dr. Lewis A. Sayre, Surgeon to the Bellevue Hospital, New York; and Lecturer on Clinical Surgery.

[Dr. Sayre considers extension the proper basis of treatment of hip-joint disease. He doubts very much whether disease ever commences in the cartilage of a joint, though this subsequently becomes involved in it. The diagnosis whether the disease has commenced in the bone, or rather in the blood-vessels at the extremity of the bone, or in the synovial membrane, or in the ligamentum teres, is sometimes difficult to make out, for the one soon runs into the other.]

Taking it for granted that I am speaking, not to medical students, but to medical men who are perfectly familiar with the physiological condition of the joint, I will go at once to the pathological changes that take place.

There is, first, a synovial inflammation, which is always followed by effusion. Then there is a wrench or tear of the ligamentum teres, which is almost always followed by destructive inflammation of the bones of the joint, particularly if a

concussion have been at the same time associated with the wrench or tear. Synovial inflammation is almost always the result of exposure. Skating, racing, jumping, playing, football, and other movements that over-exercise the joint, followed by a sudden exposure and change of temperature, will produce synovial inflammation in the hip-joint as well as any other joint. When boys are wrestling, going through violent gymnastics, twisting their legs in various positions, putting the ligamentum teres upon the stretch will produce a tear—probably not tearing it entirely off, otherwise attention would be called to it, and they would be put under treatment; but generally the smaller the rip the greater the danger, because it is neglected; and it is sometimes months before the disease is seriously developed, and frequently by this time the origin of the difficulty has been forgotten.

A concussion, a blow, a jump from a great height, would cause a pressure upon this little network of blood-vessels which I described as existing between the intra-articular cartilage and the head of the bone; this would produce a "blood-blister," or extravasation of blood at that point, which would be the nidus or starting-point of the disease; and, if the injury were detected at the time of its infliction, *rest alone* for a sufficient length of time would probably result in a favourable termination of the difficulty in great majority of instances. But, the injury not being detected, and in many instances not even suspected, the *rest* is not *insisted* upon at the proper time, and thus the disease is slowly developed, and frequently is not distinctly pronounced until so long a time after the accident that caused it, that the trifling injury which has been the origin of so much trouble has been entirely forgotten.

A pinch of the skin producing a "blood-blister," or slight extravasation in the cellular tissue, is of common occurrence, and of no great importance. If left alone it will soon be absorbed, or at most, if you let out the fluid and do not irritate the wound, it will soon get well: but, suppose even in this most trifling injury that, instead of giving it rest and time to heal, you constantly scratch it with a rusty nail, you will produce a sore, that will last as long as the irritation is continued. This is a parallel case to a joint being exercised after a concussion or wrench producing an extravasation in the tufts of blood-vessels already referred to.

The accidents to which I have referred are the three general causes of the disease which will of course be more or less modified in its symptoms according as it is developed in one or the other of the tissues referred to. If there be effusion within the joint it is always accompanied by a peculiar distortion. That

distortion necessarily arises from the fact that the capsular ligament can only hold a very small amount of fluid without being distorted. You will remember the ilio-femoral ligament that lies upon the front of the capsular ligament, running from the anterior inferior spinous process of the ilium to the trochanter minor ; it is folded over the capsule, and causes it to lie close to the neck of the bone. There is a very small amount of fluid in the capsule normally, and you cannot increase that amount without increasing its capacity by *unfolding* it. As long as the limb is in the normal position, the capsule rings round the head of the bone so tightly that no additional fluid can be placed there ; but if inflammation take place, and there be increased secretion, accommodation is made by the unfolding of the capsule, and the flexing of the limb when the capsule becomes loose, so as to enable it to hold this increased amount of fluid. That is the reason why the leg always becomes flexed, *ab*ducted, and rotated outwards. I am particular about these changes, because they materially aid you in making a diagnosis; and the thing is to be able to diagnosticate the disease in its early stages, when a great deal can be done towards its relief.

If the effusion have become very great, the limb is more flexed, more abducted, and more rotated outward, and at the same time more fixed, apparently ankylosed, as if it had been solidified by plaster of Paris ; yet it is not a bony ankylosis, but simply arises from distension by effusion and from muscular rigidity.

One of the things that is sure to take place in any inflamed joint of long standing—the hip, knee, or any other joint—is atrophy of the muscles above and below it ; and at the same time reflex irritation causes muscular tension or muscular contraction. The muscles that move the bone become irritated and contracted, so as to bring pressure against the diseased surfaces, which aggravates and complicates the disease from the beginning to the end. This is one of the great sources of pain and trouble which we have to overcome in these diseases. So in diseases of the hip-joint, the muscles contract and flex the limb, and would adduct it, but it cannot be adducted on account of the anatomical distension of the capsular ligament necessarily forcing the limb outward, rotating it outward and *ab*ducting it. Now, the adductor muscles (as all who have seen this disease must have observed) become, like catgut, intensely contracted, and feel like a strong cord. The desire to bring the limb in is on account of this reflex irritation of the adductor muscles, and the absolute impossibility of doing so is because of the capsular distension of which I have already spoken. If you take a dead body and flex the joints so as to break up the *rigor mortis*, and place it on the back so that both limbs are in their natural

position; if you now bore a small hole through the ilium into the acetabulum and forcibly inject a small amount of quicksilver, it will cause the limb to flex, abduct, and rotate outward at the hip-joint. By driving into the hole a small piece of wood to retain this increased fluid within the joint, you will find it impossible to extend, adduct, and rotate the foot inward, without rupturing the capsule; and the attempt to do so, if the capsule do not rupture, will force out your plug like a pellet from a pop-gun.

It is the struggle between the irritated muscles to adduct the limb, and the impossibility of the limb yielding to their traction on account of the over-distended capsule, that cause the pain to be so great in the *second* stage of the disease; and it is made manifest on the inner and lower portion of the thigh near the knee, on account of a little filament of the obturator nerve which runs off there to the ligamentum teres, and a sensitive branch from it running down to the adductor muscle. This over-distension can only be relieved by puncturing the joint and letting out the superfluous fluid, or by cutting the tendon, so as to accommodate the one to the other. The treatment will of course depend upon the condition in which you find the patient; you must exercise your own judgment as to which method is to be preferred, according to the condition in each case. In this stage of the disease the pain is agonising, torturing, with loss of sleep and loss of appetite. The least attempt at movement in any way causes the most agonising shrieks; and at this period ulceration takes place in the cartilage of incrustation. The patients begin to have what are called night-spasms—the peculiarly characteristic pains that occur in the night depending, I suppose, upon the fact that the child becomes completely worn out, loses its hold on the muscles, and drops off to sleep for a second; then there is a spasmodic contraction bringing this increased pressure and movement of the diseased joint, and the child wakes up with a shrill shriek. The mother runs upstairs, and finds, perhaps, that the child has dropped off to sleep again. You hardly ever ascertain this fact unless you live in a hospital, or stay for several nights in a house where there is a child with disease of the hip-joint, when you will appreciate this thing very accurately indeed. The tension of the muscles that the child is undergoing all the time keeps the joint from movement, and makes him comparatively comfortable; but that is done at the expense of vitality. The constant effort of the child to hold the muscle still to prevent movement wears him out, and by and bye he drops off to sleep from sheer exhaustion, and then, when there is the least movement, he screams as though he were pierced with a red-hot iron; instantly the muscles

are on guard again, and the child is relieved. That goes on over and over again. After awhile, ulceration takes place, and the capsule ruptures; then this imprisoned fluid escapes from the expanded capsule into the cellular tissue, and the child is comparatively easy. But the disease has still further progressed. We often think we have gained a great deal because the child is so much more comfortable, whereas the disease has only gone on to the *third* stage, in which effusion takes place into the cellular tissue. Almost immediately there is a marked change of attitude, or character of the deformity. The leg is immediately adducted, drawn close to the other; the toes are turned inward, and the limb is apparently shortened. If this rupture that takes place through the capsule be very slight, so that the pus oozes out by slow degrees, the change takes place very slowly; but if (as is often the case) there be a big rent in the capsule, you may find the child distorted in one way to-night and to-morrow the very reverse. The fluid has suddenly escaped; and that has always been heretofore described as the luxation which occurs in the third stage of hip-disease. I looked through your museum the other day and saw a great many specimens, but none are luxated. The theory of luxation in the third stage was proved by Dr. Alden March of Albany, New York, to be a mistake. The head of the bone is absorbed; the acetabulum is absorbed and increased in size; and the capsular ligament which surrounds the acetabulum slips up, and up, and up, and so it remains—the periosteal development, giving an additional growth of bone, makes a new attachment above, so that the capsule is still attached, and the head of the bone embraced in it only slipped up. You may call it, then, a displacement of the acetabulum, not a luxation of the head of the bone. The absorption taking place in the acetabulum and in the head of the bone, causes the limb of course to be very much shortened; and it being adducted, and, the neck having been nearly destroyed (in many instances entirely), the idea of luxation has arisen; but I draw a distinction between a destroyed and changed position of the acetabulum, and a luxation of the head of the bone. I call it a luxation when the head of the bone is outside the capsular ligament; but if the ligament have been displaced or moved in its position, still embracing the head, it is a displacement, not a luxation.

So much for the pathological changes that take place; next as to the causes of this disease. My impression is, that the great error we have always committed has arisen from looking at this disease as necessarily one of constitutional dyscrasia, a blood disease, a constitutional poison; that this is simply a local manifestation of a general disease. Believing this, we treat the system, we give internal remedies, and apply our

medicines for the purpose of changing the character of the blood, instead of paying attention to the local disease which is the great source of trouble. All authors, as far as I know, particularly the older ones, have looked upon this as a scrofulous or tuberculous disease of the bone, necessarily connected with some hereditary taint or some peculiar scrofulous condition of the system, and therefore they believed that the disease must necessarily go on to destruction, and that there was no necessity for any local treatment except counter-irritation, blisters, setons, and so forth. Internal remedies were the principal things relied upon. The result was, with all that kind of treatment with which we are familiar, always the same—viz., shortening of the leg by absorption of bone, and, finally, the consolidation of it in more or less deformed positions; and then the patient frequently came into a condition of robust health. I do not suppose there is any person in this room that cannot call to mind some old fellow with a shortened hip perfectly ankylosed, yet with a ruddy face, a good healthy complexion—a vigorous robust old man. If he had had scrofula in his blood, if he had had tuberculosis in his system anywhere, it would have remained there; and when the hip recovered, the man would have been a miserable old fellow after all. The fact of his being a vigorous robust man after going through all this trouble, proves, in my judgment, that the disease is not of constitutional origin. The fact that in so many cases the joint has been exsected when the patients have been apparently at the point of death, and that they have become, when the source of trouble has been removed, vigorous strong persons, is another evidence that the disease is not constitutional. Then there is the still stronger fact that, by making application to the local disease, and treating it irrespectively of any constitutional taint, you produce perfect results, so that the patients recover without the slightest deformity, and with perfect motion; that is the best proof in the world of the disease being local. Follow up the history of these cases, and you will find that it is a local disease. Out of several hundred cases that I have accurately observed, and taken the trouble to trace their history, the immense majority, I may say ninety per cent., occur in the most vigorous, robust, wild, harum-scarum children—boys that take their chances of danger, that run races, that climb over fences, jump out of apple trees, kick their playmates down stairs, ride down the banisters, are reckless and careless. The sickly, poor, scrofulous child, who clings to his mother's apron, does not run the chance of getting hurt. I do not say that scrofula is a preventive against disease of the hip-joint—by no means. All things considered, a smaller amount of injury would produce the dis-

ease in one of these miserable young ones sooner than in a healthy robust child. But such children take very little chance of being hurt, and consequently the majority of cases occur in the active and robust. The injury that starts the disease is sometimes very trifling, and not observed at the time. I might illustrate this by my own experience. In getting out of one of your cabs, I have nearly torn off my tendo Achillis. You have no kerb-stone as we have, and no steps to your Hansoms; and the result was that, expecting to step down only eight or ten inches to reach the stone, I found I had to go eighteen or twenty inches, and thus I strained the tendo Achillis. If I had torn it entirely off it would have done no damage, except loss of time, because I should have been compelled to lie by; but I hurt it so little that I paid but little attention to it; and I have been walking about for a week or ten days, until I can scarcely walk any more. It is now bad enough for me to begin to take care of it. If I had hurt it a little less, it would have been more dangerous, because it would have gone on by degrees producing those conditions that terminate in a destruction of the part.

In the first stage of the disease it is necessary to examine the child by taking off every stitch of clothes. Then, going behind, you will observe the peculiar form of the nates. The diseased side will have the gluteo-femoral fold a little dropped down; the two buttocks are not exactly alike; and you will observe, if you watch carefully, that the toe has a tendency to turn out, and the child bends his knee and hip. The sound leg is made into a solid column to receive the concussion and bear the weight of the body; the child is careful not to concuss the other at all. By careful measurement you will find that, even in the first stage, the thigh is already smaller than the other. Of course you want to examine very carefully in this stage, and it will take time to detect it; but, after examining some little time, you will see the gluteo-femoral fold dropped lower, the nates flattened, the line from the centre of the sacrum to the trochanter major a trifle greater than on the other side, and the leg having a tendency to be slightly abducted, and flexed at the knee. You then lay the child perfectly flat on the table. The case before us is a very illustrative case. You see the perfectly cautious manner in which the child moves; she sees everything, and keeps herself constantly on guard. There is now no movement at all at the hip; she is locked up, and she keeps herself as quiet as possible. The case is one that has gone on for some time [the mother stated that the child had been suffering for four years; that she had a slight fall at first, and did not complain of it at the time], and it is so conspicuous that anybody could find it out across the street. It

is in the incipient stage, such as this child was in three years ago, that you want accuracy of diagnosis. This case has gone on to the third stage of the disease.

It is essential to a proper diagnosis of these cases to have a normal stand-point to go from, and it is this. The body of a child in a healthy state can be laid upon a flat surface, the spine lying upon the floor or table ; and a line drawn from the centre of the sternum over the umbilicus to the centre of the symphysis pubis, and another drawn from the anterior superior spinous process of the ilium of one side to that of the other, cross each other at a *right* angle, when the trunk and pelvis are in a normal state. In that state, if the hip-joints be perfect, the legs can be brought so that the popliteal spaces can be made to touch the floor or table. Now, whatever position the other leg has to be kept in, in order to get these two lines at a right angle with each other, that is the characteristic deformity of the leg. A sofa, bed, or lounge, will not do for this examination —it must be done on a *solid* foundation. In this case, you see, in order to get these lines right, I am compelled to hold the leg flexed, but it is simply adducted. That is a very important point. It is an evidence that the capsule has become ruptured. If there were effusion in the capsule, the leg would be *ab*ducted and rotated outward. The fact that it is adducted, and inside the central line, as you see, is evidence that the disease has gone to the third stage, and that the capsule has become ruptured.

You see that the child, when held in the manner in which I am now holding her, has no pain. Putting my hand upon the pelvis, I make movements, and you see that the joint is not ankylosed. Seizing the pelvis with one hand, and passing the other under the knee, and making moderate extension in the direction at which the deformity has arrived, I make movements, and the child has no pain whatever. I let her go for a second, and then, on making the slightest movement, you see what pain she suffers. Now, I have ankylosed that joint; there is ankylosis perfect and complete to all appearance ; it is muscular rigidity that has produced it. You see here the disease aggravated and continued by pressure. You could not have a better illustration of the practical working of the principle of extension.

You perceive that the child can be moved about without any difficulty if she be handled properly, and the limb slightly extended. A person accustomed to it can take up a child with apparently the greatest carelessness, and will *seem* to be acting with perfect brutality, without giving any pain whatever. He knows how to apply his force ; and yet in a single instant he can produce the most agonising torture. See the way in which the child locks herself up, and makes herself perfectly rigid the instant I omit my extension.

Pressure causes absorption. By constant pressure of the head of the bone against the acetabulum, there is absorption of the head and absorption of the acetabulum; destruction takes place, and terminates in an abscess which finds an outlet somewhere; or else it goes on to ankylosis.

Many persons apply a plaster of Paris splint in these cases. This is good, so far as it keeps the part still; but, as it lacks the power of extension, it does not accomplish all that is desired. It is better to have the limb recover with motion than ankylosed.

If I take hold of the pelvis, and make moderate extension, the child is free from pain; if I put it down straight I hurt her, for I put the psoas magnus and the iliacus internus on the stretch. The straightening is to be done by slow degrees. You see the result in the case of the other child, whose leg was like this, but is now much better, from the fact that extension has been applied for some days. It is best to put these patients to bed and apply the night extension. You put the extending force on the diseased limb very nearly in the angle of deformity as you find it; lowering your extension by slow degrees, until, in the course of a week or ten days, or two or three weeks, you get the limb as straight as in the case of this other child.

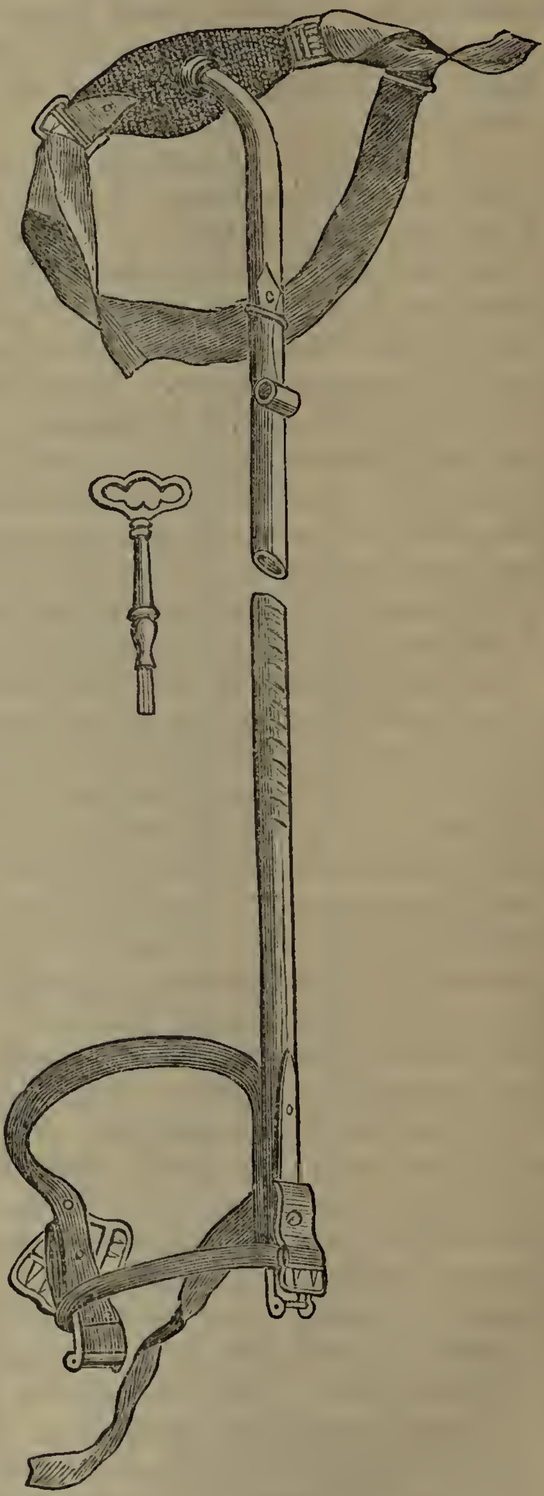
This is really the proper time to apply the instrument which I propose to put on this child in order to enable it to take out-of-door exercise. If you take it in the early stage of the disease you do not want to do that; but, this child having undergone this deformity, we must take her as we find her.

Mode of applying Extension.—We take two strips of adhesive plaister two or three inches wide (according to the age of the child), and extend them from just above the ankle on each side, and long enough to reach two or three inches above the knee, first having had a web sewed fast to the lower extremity of the plaister for the purpose of putting on a buckle for night extension. These straps are secured by a roller, as you see. I always put on the plaister without heating it. When heated, plaister is very apt to slide from itself; it moves off from the web or cloth on which it is spread. If you heat it, it burns the skin, or glides away from itself; but if it be put on with gentle pressure, this does not occur. I am careful not to make the upper end of the plaister stick before reaching it. If you put on the plaister with a fold and get a pucker in it, the effect is the same as pulling your boot on with a pucker in your stocking: it does not hurt you much till after dinner; before bed-time it is unbearable. The plaister has to be worn many months, and it should be applied carefully. I mention these little matters as the result of my own experience and observation. You will only learn by making blunders: and having made many myself,

I try to teach others to avoid them. It is remarkable how easily the plaister may be worn for months if it is smoothly and comfortably adjusted. The skin should always be thoroughly washed, and carefully *wiped* before applying the plaister. Having gone above the knee for an inch and a half or so, I reverse the plaister over the roller. At first I make the roller hold the plaister until I get above the knee; that is, simply to catch a bite. as it were, on the condyles, so that extension shall be made upon the hip, and not upon the lateral ligaments of the knee-joint. I then reverse the plaister and bring the roller back over it; and now the plaister is made to hold the roller in place so that it will not slide. This can now be worn for three or four months. If the bandage become dirty you can take it off without removing the plaisters; or you can put a long stocking over it.

FIG. 1.

Instrument of extension.



This is now ready for night extension. We next put on the plaister to apply an instrument for day extension, so that the child can run out of doors. This is done by taking two fan-shaped pieces of plaister, and sewing on to the pointed end a piece of webbing just wide enough to fit the jaws of the instrument. The instrument—(Fig. 1)—is made of two pieces of steel, one sliding into the other, as you see, and made to lock by a

key. This rod terminates in two branches that go over the thigh, and come on to the inner portion of it. I formerly made the instrument to go down the whole length of the leg; but finding that it only stiffened the knee-joint, I prefer now to make it go over only the thigh. By means of a cross branch, you take the extension from the inside of the thigh. The rod should be made to extend from an inch and a half to two inches above the condyles of the femur up to the crest of the ilium. It ends in a little roller, over which the webbing is to play, and it is capable of being elongated and shortened at will by the key. The upper portion terminates in a ball-and-socket joint in a little steel band which runs round just under the crest of the ilium. At each end of this band is attached a perineal pad, which runs under the perineum, and is buckled; and counter-extension is made on the perineum, so that the child walks on the perineal pad instead of on the hip-joint. This was a desideratum which I was always trying, for a great many years, to obtain, but did not succeed. A man named Davis was the first to make an instrument accomplishing this object. It was a piece of steel running down the whole length of the leg to the ankle. Extension was made by a joint and a buckle,but it could not be gauged, regulated, or controlled with ease or accuracy; still the principle of keeping up extension, and at the same time permitting motion, was made first by this man Davis. Had he remained an honourable man, true to his profession, he would have deserved to have his name passed down to posterity with respect; but having taken out a patent for his instrument, he has incurred the reprobation of every honest and honourable man. We will, however, give the devil his due, and admit that this man was the first who made an instrument accomplishing this object. But his instrument has been so modified and so immensely improved, that his patent is of no avail.

You lay the instrument over the thigh. If it do not fit exactly, you can bend it with the fingers; if it be a little too tight, you can spring it apart. You put your thumb and finger opposite where the two lower jaws come, and you will know the place to commence the application of your plaister for the extension of the thigh. Sometimes the pain is so intense that it is necessary to give chloroform. The plaster is put on and bandaged round, as you see, working in and out basket fashion, so as to cover any inequalities of the skin. After a few applications, you learn how to do it snugly and smoothly. By having the plaister cut in slits at the upper ends, it can fall in pieces or fall together more readily, and thus be applied more smoothly.

I used to put the bandage over the pelvis, in the form of the figure 8, but I prefer the plan of splitting it in strips and re-

versing it over the roller, as you now see. Remember that it is not requisite to pass above the trochanter major. It is hardly worth while to draw the anatomist's attention to that; but I have seen the best surgeons thoughtlessly apply the plaister clear above the joint, and of course it only pulled the pelvis instead of opening the joint.

If this instrument had been put on the child before she became so much distorted, you can well understand how much greater the advantage would have been; or had she been kept in bed two or three weeks, and the limb pulled down straight; but we will take the worst kind of case, and see whether it will do any good or not. As I am compelled to put on the instrument immediately, I press the plaister on, rather than apply it by heat; and by keeping the limb extended, I can press the plaister home without hurting the child. But it is better to leave it for a few hours to get an attachment. [The accompanying sketches (Figs. 2 and 3) show the child before and after the instrument was applied. In a few minutes after its application, the child walked around the room in perfect comfort, sat down on a chair, and got up several times to show the perfect freedom of motion in the joint.]

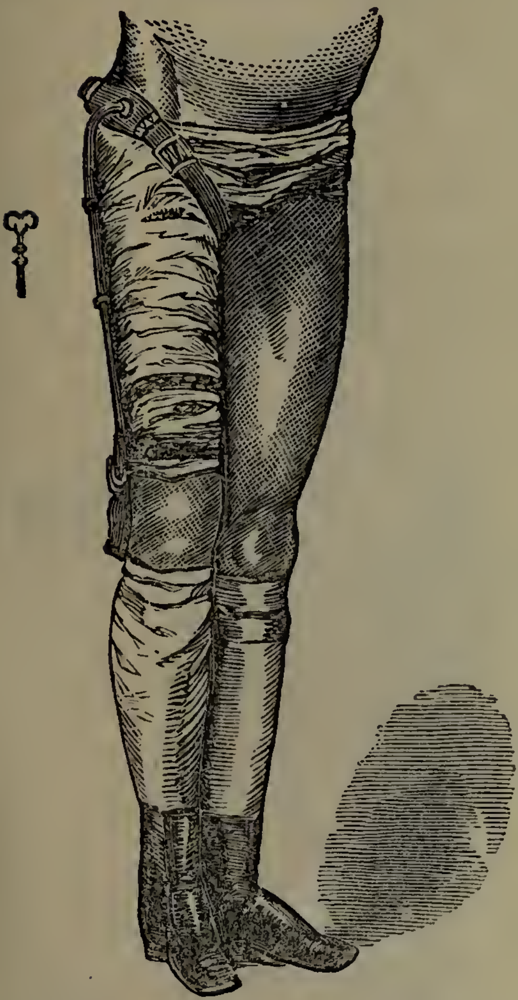
Fig. 2.

Child before the application of instrument.

This treatment may be required for months and months; and when the patient is well, I make him wear the instrument for weeks or even months afterwards, as a means of protection or guard. If the child be small, no crutches are required; but, of course, if the weight of the body be too great to be sustained by the plaister, crutches must be added. The advantage is, that the ligaments are kept in movement. The objection to the *fixed* treatment is, that the ligaments which are not involved in

FIG. 3.

Child after the application of the instrument.



the disease become diseased by rest. By giving them motion, they are kept in a normal state. A piece of old leather, if you do not handle it and keep it supple, is good for nothing.

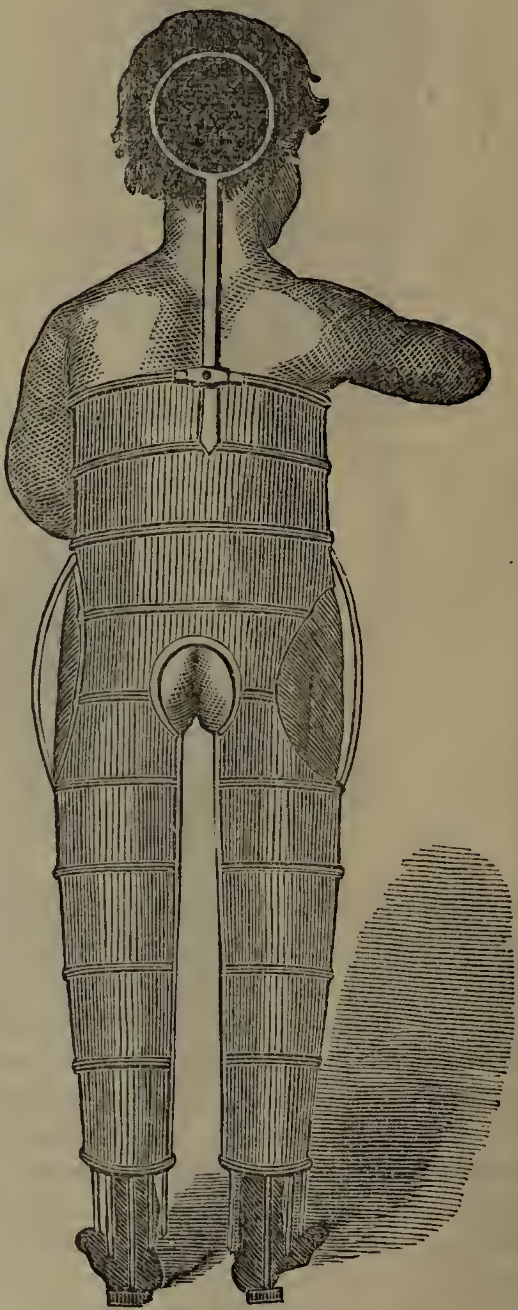
When the disease has gone on to another stage when sinuses have occurred, and discharge pus, when a probe leads down to dead bone, there is nothing to be done but to exsect it by making a small incision above the trochanter major, midway between it and the crest of the ilium, over the top of the acetabulum—a semilunar incision, the belly of the D covering the posterior part of the trochanter major, going straight down to the bone, *through* the periosteum. You then pull the soft tissues on one side, and, taking a small but strong curved bistoury, go as far around the bone on each side as you can reach, at right angles to your first incision, so as to divide the periosteum completely; you then take a strong firm periosteal elevator, with a large handle, and the end slightly curved, and go into this little triangle; you *peel* off the periosteum, and, as a matter of course, all the muscles with it; by opening the joint thoroughly, and turning the head of the bone out, the periosteum is peeled off from the inner portion; you then saw off the bone above the trochanter minor. I believe that this is better than cutting through the neck. If you go through the neck, the trochanter major comes over the hole and prevents discharges; whereas, by cutting off the trochanter major along with the neck of the bone, you leave a perfectly free opening for the discharges from the diseased joint; and by peeling off the periosteum in the way which I have described, you carry with it all the muscles that move the joint; and if you then keep the leg pulled out to its proper length, by putting on a

pair of wire breeches, you can send the patient out into the air the very next day (Fig. 4).

Here is a photograph showing the result of a case that has been exseeted. Three inehes and a half of bone have been taken away, and the head of the bone united. Here is another, in whieh four inches and a half were removed, and the man reeovered with a movable joint. This which I now show you was the first case performed in Ameriea. I operated on the patient in 1852. I invited many surgeons of New York to eome and see the operation; but no one attended, as at that time it was thought to be unjustifiable. The patient is, as you see, a handsome woman, and no one would observe any defect in her. Here, however, is the most sueeessful case, in which four inches of bone were removed. The whole aeetabulum was destroyed; the head and neck nearly absorbed. There were several sinuses leading down to dead bone. I removed the head and neek, cutting it off just above the trochanter minor; but, finding the disease extending further down, I pushed the femur up, and sawed it off again. By keeping the limb extended, if you will peel off the periosteum in the manner which I have described, the bone will be regenerated, and be as good as ever. By extending it sufficiently, you make the limb as long as before. When you have got the bone regenerated, and before consolidation takes place, you commence passive movements, and in that way obtain an artificial joint, as in the case of an ununited fraeture. By eonstant movements you prevent ankylosis; and, as the muscles

FIG. 4.

Wire-breeches.



have all been carried off with their insertion at the periosteum, you have certainly got the normal position from them to be moved again. The proof of it is in this boy. He bends his leg at right angles; he can kick higher than his forehead; and he won a pair of silver skates two years ago, in a contest with some very good skaters, on the Central Park pond in New York. The leg is almost exactly of the same length as the other, and the motions are perfect; and yet here you see four and a half inches of his femur, and the acetabulum was perforated.

I have been rather surprised to receive a letter from Mr, Barwell, who said he was going to publish it in the *Lancet*. I hope, for his own sake, that he will not. He dislikes my plan of removing the periosteum. Of course every one has the right to select any plan that suits him best. I am extremely reluctant to be dragged into controversy; and, without answering anything of the kind, I will draw your attention to the answer which you can make yourself. If you take an orange and cut a slit down through the skin, I should like to see any of you with a sharp penknife peel the orange out of the skin without splitting the skin or sticking the knife into the orange; or if you take a straight broad knife, like a scalpel, and take the orange out without damaging the skin or making an occasional hole in the orange, you are more dexterous than I am. But if you will take a knife and cut across the skin, you can take the handle of a silver spoon, slip it in, and peel round under the skin, and separate the orange from its covering without tearing it. So it is in regard to the femur. In *dissecting* off, you tear the periosteum into shreds, destroy its vitality, and prevents it doing the good which you want it to do. By commencing *above*, and "prying" it off, without first limiting its extent by the circular incision above referred to around the femur, you are in great danger of loosening its attachment to the bone below the point where it is to be sawed off—where you do not want to loosen it. You want the periosteum left attached to the femur. Therefore I prefer cutting down over the trochanter major, through the periosteum, down to the bone, as far as opposite a line a little above the trochanter minor. Taking a small bistoury, and going at right angles to this first incision, half-way round on each side, I thus limit the line of removal of the periosteum to the place where section of the bone is to be made; and below that point the periosteum remains attached to the bone. Since adopting this plan of operating, and always using the saw to divide the bone, instead of the pliers or bone-forceps, I have not had exfoliation follow my operations, as in former instances. To prove that this is the better way, I may mention that I have

operated forty times, and thirty of my patients are walking about with excellent movement of the joint; and you here see daguerreotypes of them. Eight or ten of them, I am confident, might be walked into this room and not one gentleman here would be able to say which leg was operated on. These are cases in which I have operated in the way I have described. I did not learn to do this the first time I tried; and some of the cases, therefore, did not get on so well. I have had some short legs; and I have had cases where there was a good deal of exfoliation, because I did not peel the periosteum properly; and I have had exfoliations because I cut off the femur with a forceps, which in my judgment ought not to be done.

Let me, before I conclude, say a word on operative surgery and anatomical surgery. You sometimes see beautiful dissections. Men will hold the knife like a pen, lay out the various tissues, and give a very pretty dissecting-room demonstration. Every surgeon should go through this anatomical surgery in the dissecting-room, and do it thoroughly and frequently; but when he comes to apply his knowledge to the living human being, let him be an *operating* surgeon, and go straight to the place which he wants to reach, without splitting hairs and dividing endless series of fasciæ.—*British Medical Journal, July* 22, 1871, *p.* 85.

31.—ON EXCISION OF THE HIP-JOINT FOR DISEASE.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

What are the changes which the joint and constitution undergo in the course of natural cure by anchylosis? In the joint there is a twofold process of destruction and reparation. A piecemeal or molecular excision, so to speak, is constantly progressing, apparently by absorption, and certainly by the draining away of débris of bone in the discharge until two healthy opposed surfaces are reached, so that union may at length be effected. This natural cure of joint disease entails a protracted period of recovery, extending even to many years, as compared with that of weeks or months required for recovery after excision. During this ordeal the constitutional vigour is reduced, subsequently leaving the patient stamped with the aspect of suffering in former years. Occurring also as it often does during the growing period of life, the reserve power, which should have been gained to meet the exigencies of after-life, is used up prematurely by incessant demand in the long process of reparative anchylosis. Persons who have undergone the natural cure of diseased hip-joint, for example, may be seen hobbling about the streets, being easily recognised

by the characteristic gait of old-standing hip disease, and by their sallow and prematurely aged appearance. This constitutional decrepitude may possibly be averted by a remarkable acceleration of the excisional part of the process. In a case represented by specimen No. 7 of the hip-joint series (exhibited before the Society), an eminent surgeon differed with myself and others respecting its diagnosis; nature subsequently undertook the operation of excision *en masse*, for she severed and discharged the greater portion of the head of the femur through one of the fistulous tracks. This natural excision of a joint—one of the only two, I believe, on record—will be singularly suggestive to operative excisionists, and it should be equally admonitory to those surgeons who blindly oppose the operation.

The conditions of hip-joint disease appropriate for excision.—It was formerly held, and may still be maintained by some surgeons, that excision of the hip-joint for disease should be resorted to only in the following conditions, constitutional and local:—

(1) Only in the last stage of the diseased or of constitutional endurance.

(2) Only when the extent of disease is limited, the acetabulum being free from disease and the amount of pelvic disease trivial.

(3) Only when the head of the femur is dislocated.

The reverse of these rules, or nearly so, may be partly gathered from my cases, and can, I believe, be justified by accumulated experience, drawn from the results of a large but varying number of cases, with regard to each such rule in question.

Of the three conditions laid down respecting excision of the joints in general, the first only applies to the hip-joint.

(1) Destruction of the articular cartilages, without the supervention of anchylosis, will always justify operative interference by excision. The constitutional condition will probably *not* then have advanced to hectic and emaciation. But the state of the general health should primarily determine the necessity for excision in all cases, and not any arbitrary consideration of the period of the disease and the condition of the joint. Whenever, therefore, the general health is manifestly failing, whatever may be the stage of the hip-joint disease, excision should be resorted to, and without further delay. This guiding rule was strongly urged and clearly illustrated by Mr. Hancock in his recent lectures at the Royal College of Surgeons. On the other hand, the most extreme state of constitutional exhaustion, previous to the operation of excision, may be followed by recovery after removal of the diseased bone—as the successful results in my own series of hip-joint cases, 1, 2, 4, and 6, clearly show.

(2) Osseous anchylosis with malposition of the limb will *not* justify the peril of attempted excision. Section of the neck of the femur is practicable, whereby the limb can be brought down to a straight position. This principle of operation was lately practised in a case by Mr. W. Adams, and successfully.

(3) The *extent of bone* diseased may be considerable, and involve both the femur and acetabulum. In the *femur* the diseased portion may include the head, neck, great trochanter, and shaft, entering even into the medullary canal. In the *acetabulum* the diseased portion may include the whole floor of this cavity, and even extend to adjoining portions of the ilium, pubes, and ischium. Neither of these conditions of extensive osseous disease prohibits excision; but the acetabulum not unfrequently recovers itself when the diseased head of the femur has been removed from further contact and attrition.

(4) Dislocation is unfavourable for excision, as implying an advanced stage of the disease constitutionally. The significance of this local condition will therefore diminish in proportion to the absence of marked hectic and emaciation.

Operation.—Excision of the hip-joint was originally proposed by White, of Manchester, in 1769; but the operation was first performed by Schmalz, of Pirnie, Saxony, in 1816, and first performed in this country, and for the second time in Europe, by Anthony White, of the Westminster Hospital, in 1821. It was repeated by Hewson, of Dublin, in 1823, after which period the operation fell into disuse until its revival by Sir William Fergusson in 1845. Since that period it has been performed by many surgeons, and in a large number of cases.

The hip-joint, deeply placed owing to the neck of the femur, is reached most conveniently by a T-shaped incision; the vertical line, perhaps slightly curved, being made from just above the great trochanter downwards on the shaft to about three inches or less in extent, and the transverse line about half that extent on the summit of the longitudinal incision. The very limited extent of this latter incision avoids the femoral vessels anteriorly, and the crural nerve posteriorly. In disease of the joint, with perhaps consequent dislocation backwards on the dorsum ilii, and wasting as the result of long-standing disease, these incisions seem to be almost invited, so prominently does the trochanteric portion of the femur abut under the integument. By detaching the integument on either side of the vertical incision, keeping the knife turned towards the femur, especially on its inner side, the subjacent portion of femoral shaft is exposed; then, sinking the knife vertically in the transverse incision, just above the trochanter, the attachment of muscles thereto is divided, so that the finger can be readily passed down to the joint and its state ascertained. The capsular ligament

will generally have given way or entirely disappeared. To turn out the remnant head of the femur for excision it may be necessary to adduct and evert the limb, when, with a touch of the knife on the bone, the round ligament yields and the head starts from its socket. Or this ligament also may have disappeared, and the head and neck of the femur be so reduced, and the acetabulum so patulous, from more advanced disease, that the bone can be readily dislodged and hooked out with the finger. In a third class of cases dislocation backwards has taken place. In any case, however, adduction of the limb across the opposite thigh presents the bone for application of the saw; and then the diseased portion is removed by one or more successive slices, the integument on either side being protected by a curved spatula. The chain-saw may be used by those who prefer it. A gouge may be used to finish off the femoral excision, instead of unnecessarily removing any healthy portion of the trochanter, if that be left, or of the adjoining shaft. The acetabulum should be scraped rather than gouged, to remove any carious or denuded portion; or more extensive pelvic excision may be necessary, and has proved successful. But superficial caries, acetabular or pelvic, will often recover itself, the former having been maintained by constant attrition of the femoral head. Any hemorrhage is easily arrested by torsion. I have never had occasion to apply a single ligature in any hip-joint excision.

Excision of the trochanter major may occasionally prove sufficient, caries of this portion of the femur existing without disease of the hip-joint. I have had one such case, and with a successful result.

The *after-treatment* of excision, whether of the hip-joint or of the great trochanter alone, is very simple. The limb may be laid straight in bed, and retained in position only by a small side pillow or roller sand-bag, without absolutely fixing the thigh. Or a long splint may be applied, extension being made from the opposite thigh, as recommended by Sir W. Fergusson. Of these two modes of after-treatment I prefer the former, especially for the joint-operation. The section-end of the femur is drawn up by muscular action, and hitches just above the acetabulum, which, having been generally more or less superficially carious, is thus left to recover itself, undisturbed by any attrition of the femoral end of bone; while a new and firmly fibrous movable joint forms where the end of bone rests above the acetabulum. There is little or no tendency to displacement after hip-joint excision, and the slight extra shortening which results from thus leaving the limb to itself is unimportant compared with the advantages in regard to the acetabulum, and the formation of the best kind of new joint requisite for the func-

tional use of the limb in progression, as well as for support. All my cases were treated in this way, and with perfect success.

Results.—(1) In relation to life or mortality. In 111 cases collected by Dr. Hodges of *unrecorded conditions* of operation, 56 recovered, 53 died from the combined effects of the operation and the previous disease, and in the remaining 2 cases amputation was resorted to. Thus about 1 in 2 died—a very high mortality. But Mr. Hancock presents the following very interesting results as to the mortality with reference to certain *guiding conditions* of disease for operation :—The *acetabulum*, in a healthy state, gave a mortality of 6 in 18 cases, or 33 per cent. On the other hand, acetabular *disease* has had more favourable results of operation. Of the 10 cases in which perforation had taken place, 6, or 60 per cent., recovered ; 2 only, or 20 per cent., died. Of the 4, in which not only perforation existed, but abscess also within the pelvis, 2 recoverèd, 2 died, —50 per cent either way. Of the 3 in which the acetabulum was trephined for the evacuation of matter from the pelvis, 2, or 66 per cent., recovered ; 1, or 33 per cent., died. Therefore, in the whole 20 of these apparently most unfavourable cases for excision, the mortality was only 5, 1 in 4, or 25 per cent. *Dislocation* of the head of the femur having taken place, the per-centage of recoveries was actually 46, against 23 where it had remained in its socket ; the total number of cases compared being 143. Of my own 8 cases of hip-joint excision, in 4 there was dislocation, and they all recovered equally with the 4 in which dislocation had not occurred.

Another equally large series of cases—112, British and foreign—has been collected by Dr. R. R. Good, late surgeon in the Confederate American army. This series is the more complete as it embraces the most *essential particulars* respecting excision of the hip-joint for disease, both in regard to its mortality and the state of the limb. We are thus enabled to take a commanding view of the whole subject.—*Lancet, July* 15, 1871, *p.* 77.

32.—THE APPROPRIATE SPLINTS FOR USE AFTER EXCISION OF THE KNEE-JOINT.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital.

At the London Hospital, for some time past, stout hatter's felt (as used for the hats of the Cornish miners) has been largely employed for these splints. It is a substance very readily cut and moulded to the limb after soaking in hot water, and capable of easy adjustment with iron brackets, etc., where

such appliances are required. We saw several such splints in the wards, and they seemed to be excellently adapted for their purposes, being quite firm and yet light, and comfortably moulded to the joints supported by them. Mr. Hutchinson mentioned that, in his last three cases of knee-excision, he had modified the operation somewhat by peeling up a skin flap, after making the customary horse-shoe incision, and so getting into the joint above the patella, and removing all the synovial membrane and ligamentous tissues in front of the bones; and in these cases the wounds had healed very favourably.—*Medical Times and Gazette, July* 1, 1871, *p.* 8.

33.—ON FRACTURE OF THE PATELLA.

By Jonathan Hutchinson, Esq., Surgeon to the London Hospital, the Blackfriars' Hospital for Skin Diseases, and the Royal London Ophthalmic Hospital.

In most patients suffering from transverse fracture of the patella, if we examine the bone several hours, or a day or two after the accident, we find the fragments separated from one another by a considerable interval, from a quarter of an inch to an inch. We also find, most likely, considerable effusion into the joint. At first thought we shall, perhaps, be inclined to ascribe the separation of the fragments to contraction of the rectus muscle; but that it is really due to quite another cause will be apparent when we examine a little further. We shall probably find, in the first place, that the rectus muscle is not in a state of contraction, or that it becomes so only under the pressure of the fingers or at the patient's will. It is true that muscles possess a certain small amount of *elasticity* in addition to their *contractility*, and this elasticity, when unopposed, will tend to draw the fragments apart; this force is, however, comparatively unimportant, and for practical purposes it may be safely neglected. We must bear in mind that the patella is firmly attached, by its entire circumference, to tendinous or ligamentous structures of great strength. Now, when the bone is broken across the fibrous attachments on each side, the tendons of the vasti remain entire, and they are amply sufficient to hold the portions of the patella in place, and prevent their separation, so long as there is no distending force exerting pressure from within. Soon, however, fluid is effused by the synovial membrane, the joint is distended, and what was before a mere fissure through a bony part of the joint-capsule, becomes a wide gap. This is the state in which we generally find the knee-joints of patients admitted with fractured patella. In a few cases, however, scarcely any effusion takes place, and

it is precisely in these rare cases that the broken portions of the patella never become separated to any appreciable extent. Those cases in which the joint does not become distended, and the fragments are not separated, are the most favourable of any as regards good union; the case will be unfavourable just in proportion to the *amount* and *duration* of the effusion—*i.e.*, the extent and duration of the separation of the fragments. The further the fragments are apart the more difficult will it be to bring them together; and the longer they remain separated, the less likely are they to unite by bone.

You will readily see, then, that the principles of treatment are to prevent effusion if it has not occurred, to favour absorption of fluid which has been poured out, and to bring the fragments of the patella into close and permanent apposition.

It is unnecessary that I should go into detail on any but the method of carrying out the last-named principle.

The plan which I invariably adopt, and which has been carried out in the case before us, is to put the limb on a straight back-splint, and by that means bring the lower fragment as high as possible, and by pieces of cross-strapping maintain it there; then to bring down the upper fragment, by strapping and firm bandaging, as near as may be to the lower fragment, and keep it there, by re-adjusting the strapping as often as it gets at all loose. Many surgeons, after doing all this, proceed further to elevate the whole limb into the air, with the intention of shortening the distance between the origin and insertion of the rectus muscle. I never adopt this practice, because I believe it to be quite useless, and very uncomfortable to the patient. The advocates of the elevation plan defend it by appealing to a constant state of partial contraction of the muscles, which is supposed to be mainly instrumental in causing the separation of the fragments; and they assert that by lessening the distance between the ends of the muscle they diminish the consequence of this contraction.

I do not for a moment deny that the origin and insertion of the rectus femoris muscle are brought nearer together by elevating the thigh, or by raising the body; I assert, however, that the muscle when left to itself is not in a state of constant contraction; but, on the contrary, that it very soon relaxes completely; and that, therefore, any arrangement for shortening the distance between its attachments is uncalled for. I repeat, also, what I have already mentioned, that it is not to contraction of the rectus, but to synovial effusion, that the separation of the fractured portions of bone is due, and that it is, therefore, useless to make any special provision for insuring the relaxation of the muscle. I am even of opinion that elevation of the limb may be injurious, for we place it in a constrained

position, and I think that muscles when in positions of discomfort are more likely to take on irregular and violent action than when allowed to rest in their ordinary postures.

A few words as to the mode of repair in transverse fracture of the patella. I am inclined to think that bony union is not so rare as it is supposed to be. I have dissected one specimen of union by bone, and I have seen several cases in which I had not the slightest doubt that bony union had taken place. The last case that I discharged from the hospital was, I believe, one of this kind. I have quite lately seen a gentleman who was treated by his brother, a surgeon, twenty years ago, for fracture of the patella, and whose limb has during that time been in every respect as good as it was before the accident. There is in him a groove across the patella marking the seat of fracture, but, on examination, I felt no doubt whatever as to the repair having been by bone. The most favourable cases, as I have before mentioned, are those in which no effusion occurs, or in which the effusion is comparatively slight, and disappears rapidly, and which are treated by prolonged rest in bed, these being the cases in which there is little, if any, separation of the fragments. When the fragments of the patella are separated widely, and for a long time, then bony union is very unlikely to occur.—*Medical Times and Gazette, August* 19, 1871, *p.* 213.

34.—ON EXCISION OF THE ANKLE-JOINT FOR DISEASE.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

[By excision of the ankle-joint, the writer means the removal of the articular surfaces of the bones which enter into the formation of this joint—namely, the lower articular ends of the tibia and fibula, and the upper portion of the astragalus.]

This operation, first performed for *injury* by Hippocrates, and revived by Hey, of Leeds, in 1766, was first performed for *disease* by Moreau, senior, in 1792; then by Moreau, junior, in 1796; next by Mulder, in 1810; and probably by Champion, in 1813. But the credit of introducing the operation into British surgery is due to Mr. Hancock, who excised the ankle joint for disease, in February, 1851. The operation has since been resorted to by Professor Humphry, of Cambridge, in four cases, and by other surgeons, including myself.

Conditions appropriate for excision.—Disease of the ankle-joint, according to Mr. Hancock, frequently commences in the synovial membrane, and extends to the articular surfaces, thus, secondarily, involving the bones. But it may also commence, as scrofulous disease, in the cancellous tissue of the long bones,

tibia and fibula, or of the astragalus, or of all three bones. In the former condition, the affected portion of the articular surfaces may be removed with safety and advantage, In the latter condition, excision is not inappropriate.

Other and different opinions are held by some surgeons of repute, as by Mr. Furneaux Jordan, of Birmingham, both with regard to the origin of disease of the ankle-joint and the propriety of excision. That disease rarely extends from the synovial membrane, but begins in the cancellous tissue, either of the extremities of the tibia and fibula, or in that of the astragalus; and, it is said, necessarily affects the whole of that bone. In the one condition, excision of the extremities of the long bones, as the local source of the disease, is said to be inadmissible; while, in the other condition, the whole bone—astragalus—must be excised. The former contra-injunction is plainly at variance with the established practice of excision in disease of the knee-joint, under strictly analogous circumstances—when the end of the femur, or head of the tibia, is the seat of disease. Excision of the extremity of either of these long bones is not inadmissible, but only, perhaps, less favourable in scrofulous disease of the cancellous tissue.

The fair inferences from this diversity of opinion respecting the diseased conditions of the ankle-joint for which excision is appropriate, would appear to be:—

(1.) When disease commencing in the synovial membrane has extended to and destroyed the articular surfaces of the tibia and fibula, that of the astragalus, or of both opposed surfaces.

(2.) When disease, having the same articular consequences, commenced in the cancellated tissue either of the long bones or of the astragalus, provided it be limited to part of this bone—its upper articular portion.

(3.) Whether the disease originated in the synovial membrane or in the articular cancellated tissue, resulting in destruction of the cartilages without anchylosis, excision should be resorted to *before* the supervention of constitutional exhaustion.

Operation.—Hancock's description of excision of the ankle-joint, as first performed by him in England (1851), is as follows:—"I commenced an incision behind, and about two inches above the external malleolus, carrying it forwards beneath that process, across the front of the joint, and terminating about two inches above and behind the inner malleolus. This incision included the skin, without implicating the tendons or their sheaths. The flap thus formed was dissected up, and the peronei tendons were detached from the groove behind the fibula, and cut through, as was the external lateral ligament close to the fibula, with a pair of bone-nippers. I next

divided the fibula about an inch and a half above its inferior extremity, and, cutting through the inferior tibio-fibular ligaments, detached the external malleolus. Turning the leg on to its outer side, I cut through the internal lateral ligament, carefully keeping the knife close to the end of the tibia. to avoid the posterior tibial artery. The tendons of the tibialis posticus and flexor communis were then detached from the groove behind the internal malleolus, and, taking the foot in both hands, Mr. Avery holding the leg, I dislocated the foot outwards, thus bringing the end of the tibia with the internal malleolus prominently through the wound. These were removed with a common amputating saw, applied half an inch above the horizontal articulating surface of the tibia, the soft parts being protected by a spatula; the upper articulating surface of the astragalus was also removed by a metacarpal saw, held horizontally. The foot was then restored to its proper position, the cut surface of the astragalus being adapted to the cut surface of the tibia; and the wound having been closed by sutures, except on the outer side, left open for the free escape of discharge, the leg was placed on an external splint, having an opening corresponding to the wound."

The parts divided by this operation were—the skin, peronei tendons, internal and external, lateral and inferior tibio-fibular ligaments, and the articular surfaces of the bones. In subsequent operations the tendons were preserved entire. In no instance have the tibial arteries, anterior or posterior, been wounded; and never has it been necessary to apply a single ligature.

Lateral incisions alone, without the anterior communication, have since sufficed. This was my mode of operation in two cases.

The *after-treatment* of ankle-joint excision should be conducted in accordance with the principles laid down respecting the excision of other joints, regard being had to the kind of anchylosis necessary for the use of the limb. The fixed position of the foot must be maintained during the period requisite to secure a firm anchylosis of the osseous surfaces made by excision—the lower ends of the tibia and fibula, and the upper surface of the astragalus; thus to adapt the foot for support and progression. The wounds, one on either side of the ankle, are dressed from day to day, but the splint should be removed and reapplied *very seldom*. In my own excisions of the ankle joint I have fixed the leg on a flat back-splint, provided with a foot-piece; thus secured, displacement can scarcely occur, and, the side wounds are freely accessible for cleansing and dressing. Eventually a starched bandage may be applied; and the foot being slung from the neck, the patient gets about on crutches as the foot is gradually brought into use.

Results.—Excision of the ankle-joint for disease presents some most interesting results, both in relation to the mortality of this operation and the state of the foot, with the probability of secondary amputation; both these aspects of the operation being considered also as compared with amputation of the leg, and with Syme's and Pirogoff's operations of amputation at the ankle-joint, severally, for disease.

For certain valuable statistics bearing on these important questions I am indebted to Mr. Hancock, who liberally placed at my disposal the manuscript of his lectures at the Royal College of Surgeons.

In relation to Mortality.—The results of 32 cases of excision of the ankle-joint for *disease* have been collected by Mr. Hancock; all that he could find recorded in the practice of British surgeons. Of these 32 cases, 7 died—about 1 in 5, or a fraction above 21 per cent. But of the 7 deaths, 4 are reported to have died of consumption, one suffering from that disease at the time of operation; while another died of secondary syphilis. This reduces the average mortality to 1 in 16, or about 6 per cent. Of my own 2 cases, both lived.—*Lancet, Aug.* 5, 1871.

35.—ON EXCISION OF THE TARSAL BONES FOR DISEASE.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

Excision of the tarsal bones comprises certain recognised operative procedures which correspond to the lines of the osseous articulations; as removal of the astragalus, or of the os calcis. But the plan and performance of excisional operations on the foot should not be restricted by these anatomical limitations. The modification of these operations on the foot, according to the kind and extent of the disease, well illustrates the guiding principle of excisional surgery; and this application of the general principle I have laid down is specially advocated by Mr. Hancock, in the lectures already referred to.

The *conditions of disease* for which excisional operations on the foot may become appropriate are similar to those which affect other bones and their articulations; namely, the destructive result of inflammation, and especially in the form of caries. According to the extent of such disease, partial or complete excision of any one or more tarsal bones may be necessary. I proceed to describe the various operations, and to estimate their value by their results.

1. *Excision of astragalus.*—*Partial* excision of the astragalus, for disease, was first attempted by Severin in 1646, and in England by Ramsay in 1792 Since that time it has been performed by other surgeons, but in a comparatively few authentic

cases. *Complete* excision, for disease, was first performed by Mr. Busk in 1850. Three other cases only have since been recorded in England; one each by Mr. Erichsen, Mr. Holmes, and the late Mr. Statham. An incision along the outer and anterior aspect of the ankle will expose the bone; its neck should then be severed with strong cutting pliers; and, some space having been thus made, the bone may be drawn out of its bed by the lion forceps, the knife being used to detach its ligamentous connexions, but applied cautiously towards the inner side of the joint in proximity to the plantar arteries. It may be necessary to extirpate the bone piecemeal when its substance breaks down in a carious state; it must then be gouged out.

Some of the *anterior* tarsal bones may have to be removed *with* the astragalus. Liston's operation on the foot—as the excision thus extended, in a noted case, might be named—consisted in the removal of the astragalus, scaphoid, and two cuneiform bones. The case is reported in the *Edinburgh Medical and Surgical Journal* of January, 1821.

Results.—Of *partial* excision of the astragalus, in 27 cases recorded, 8 were operations for disease (caries); 5 terminated well; 1 ended in anchylosis, and in 2 the result was not stated, *Complete* excision: In 109 cases of complete excision of the astragalus, 14 were operations for *disease*, 13 being for caries, and 1 for necrosis. Of the 13 cases 1 died, 8 recovered with good and useful limbs, 2 underwent secondary amputation two years after excision, and both recovered; in 2 the results were doubtful. The case of excision for necrosis did well.

(2) *Excision of os calcis.*—*Partial* excison seems to have been first performed for injury, in the removal of a musket-ball from the os calcis, by Formius, as long since as the year 1669.

Complete excision was first performed, and for disease (necrosis), by Mr. Robert of Prague, in 1837. The second operation, and the first in this country, was by Mr. Hancock, in May, 1848. The first successful case in England was by Mr. Greenhow, of Newcastle, in the same year, 1848; and two equally successful cases of complete excision, in the hands of that surgeon, followed also in that year. Since then the operation has been practised, with varying success, by other surgeons, principally by Sir William Fergusson, Teale (Leeds), Humphry (Cambridge), Cann (Hereford), Pemberton (Birmingham), Potter, (Newcastle), Mr. Holmes, Mr. Erichsen, and by myself.

The operation is thus performed:—The patient lying on his face, with the sole of the foot uppermost, an incision with a stout bistoury may be commenced at the calcaneo-cuboid articulation, just above the sole of the foot, and carried backwards round the heel forwards to an equal point on the inner

side. This *sole-flap* of integument is raised forwards from the under surface of the os calcis, and in its whole thickness down to the bone, so as to form a good cushion ; a slight perpedicular incision is made opposite the tendo Achillis into this horizontal incision, and with a little reflection of the integument to either side the tendon is severed from its insertion. The knife is then entered posteriorly over the upper surface of the os calcis, and the strong interosseous ligament connecting it with the astragalus is divided, somewat as an oyster is opened. The bone can then be raised, and its lateral attachments being cleared by a gentle application of the knife, the calcaneo-cuboid articulation is opened, and the bone completely detached.

Or, an incision may be made, as for Syme's amputation at the ankle-joint, and a *heel-flap* having been formed, the os calcis is exposed, lateral incisions are continued, on either side, along the sole of the foot to the line of the calcaneo-cuboid articulation, whereby a short *sole*-flap is reflected forwards, and the operation completed as before. The objection to this plan of excision is the liability to sloughing of the heel-flap in disease of the os calcis ; fistulous openings frequently having formed posteriorly, where the flap is attached. The advantage of a heel-flap is that the excision can be readily converted into Syme's amputation in the event of the astragalus being found to be extensively involved. This method, therefore, was recommended by Mr. Teale, and practised by Mr. Page, as precautionary proceeding in excision of the os calcis.

Apart from the contingency of sloughing, I prefer the sole-flap method of excision.

3. *Excision of the os calcis and astragalus.*—Wakley's operation is performed much in the same manner as the second method of excision of the os calcis alone. The only differences are, an *additional* incision between the malleoli posteriorly, curving down to the insertion of the tendo Achillis, so as to expose the astragalus on raising this flap of integument, and that the integument of the heel between this incision and the incision across the heel, from malleolus to malleolus, forming the heel-flap in the other operation, is here removed. The bones are excised through the gap thus made posteriorly, by dividing the tendon and lateral ligaments, lateral incisions being continued along the foot to the line of the calcaneo-cuboid and astragalo-scaphoid articulations, so as to give access to these articulations. The malleoli are removed with bone-nippers. The posterior tibial artery must be ligatured, but the anterior tibial is avoided by very cautiously using the knife in removing the astragalus in front of the tibia.

This excision would probably meet the difficulty of disease extending *up* to the ankle-joint, and in such case prove a sub-

stitute for Syme's amputation at the ankle-joint. The operation was performed by Mr. Wakley at the Royal Free Hospital, in December, 1847, and the case is reported in the *Lancet* of July, 1848.

Results.—Of *partial* excision of os calcis, in 42 cases collected by Mr. Hancock, 38 were operations for *disease*—25 caries, 12 necrosis with sequestra, and 1 necrosis. Of the 25, 1 died the day after operation of diarrhœa, 14 recovered at periods varying from six weeks to six months, 2 required a second operation, but recovered, and 1 underwent secondary amputation, while of the remaining 7 the result is not stated. Of the 12 cases, 3 recovered, 1 required a secondary amputation, and of 7 the result is not stated.

Complete excision: Of 18 cases (the particulars of which are authenticated,) in 14 the disease was scrofulous; and in 11 of these this condition was entirely constitutional, in 1 it is said to have originated from a nail having been run into the heel, and in 2 to have followed sprains. Of the 11 cases, 1 died of diphtheria, 7 recovered, and with perfect use of the limb, while 3 suffered secondary amputation, 2 from recurrence of disease in the remaining tarsal bones, and 1 owing to erysipelas. Of the 3 cases induced by injury, 2 recovered completely, and the result of the third was doubtful.

The *general* results of complete excision of the os calcis are thus represented in 34 authenticated cases: 1 died of diphtheria, 25 recovered completely, 4 underwent secondary amputation, and of 4 the results are not given.—*Lancet, Aug.* 5, 1871, *p.* 185.

36.—ON EXCISION OF THE ELBOW-JOINT.

By Frederick James Gant, Esq., Surgeon to the Royal Free Hospital.

[The elbow-joint appears to be not unfrequently amenable to treatment without operative interference, in the destructive stage of inflammatory disease, by ulceration of the articular cartilages. The natural cure by anchylosis supervenes more readily than in other joints.]

Conditions of Elbow-joint Disease appropriate for Excision.—The three conditions which severally determine the propriety of excision with regard to the joints in general are applicable to the elbow-joint.

1. Functional inutility of the limb, depending on disease of the joint having resulted in destruction of the articular cartilages, without the supervention of anchylosis, will always justify excision; care being taken that the constitutional condition shall *not*, if possible, have approached to exhaustion. But the degree of reserve power requisite for recovery is much less than after excision of the knee or hip, owing to the average

period of reparation being less by one-half, or about six weeks instead of three months.

2. Osseous anchylosis, and particularly in connection with a useless position of the limb, will also justify excision.

3. The structural conditions of disease pertaining to the elbow-joint, which specially affect the propriety of its excision, relate to the *extent of bone* destroyed by disease. The limits of excision of the elbow-joint are not restricted by two of the three considerations respecting the knee-joint. Thus, the length of the portions of bone removed from the elbow is comparatively unimportant, the corresponding loss of length in the arm not much impairing the use of this member eventually; nor is it of consequence, therefore, to observe the epiphysial lines affecting the subsequent growth of the bones. But it is equally requisite in the elbow as in the knee to preserve sufficiently wide surfaces for the formation of an adequately secure union; in the one case, with ligamentous mobility; in the other, with osseous consolidation.

The removal of only a thin superficial section of the articular ends of the bones in the elbow-joint, leaving the section ends too nearly in contact, is apt to be followed by osseous union and an unsuccessful result of the operation. On the other hand, any new bone which may not unfrequently have been produced in the form of a spiculated enlargement of the articular ends *above* their diseased portions, and thereby limiting the disease, should not be included in the excision. The importance of observing this limitation of the operation is particularly urged by Mr. Butcher in his work on Operative and Conservative Surgery.

Operation.—Excision of the elbow-joint, for disease, was originally performed by Justamond, of the Westminster Hospital, in 1775, as a partial operation, in which the olecranon and two inches of the ulna were removed. Complete excision was first performed by Moreau, senior, in 1794, and by Moreau, junior, in 1797; but the operation attracted little attention until it was revived by Stansfield, Chorley, and Hey, of Leeds, in 1818-19, and especially by Syme in 1830, since which period it has been more generally practised than the excision of any other joint for disease.

The operation is thus performed:—A single linear incision, longitudinally over the centre of the joint, and of sufficient length to turn out the bones, is preferable to any other, in relation to speedy recovery after operation. Other forms of incision offer certain special advantages. An H shape exposes the bones more readily, on reflecting the flaps, thus marked out, upwards and downwards; and this incision is particularly recommended by Sir W. Fergusson. A ⊣ shaped incision, with

the vertical line parellel to and a little outside the ulnar nerve, allows of its being more surely guarded or drawn inwards, by a curved spatula in the hands of an assistant. In either case, the transverse line of the incision should be made across the end of the olecranon from condyle to condyle; or nearly to the inner condyle in the latter form of the incision; and thus the joint is laid open. But the single longitudinal incision is now, I believe, generally practised; and I rarely find any other more convenient. With a little detachment of the integument on either side, the knife is entered transversely above the olecranon, dividing the tendon of the triceps (the ulnar nerve being protected); the olecranon process must then be sawn off transversely, and by forcibly flexing the arm, and perhaps dividing the lateral ligaments with a light touch of the knife, the articular surfaces of the three bones are fully protruded and exposed. The articular surface of the humerus between the condyles is excised by means of a small saw; and the remaining sigmoid surface of the ulna and head of the radius in like manner, or removed with pliers, or simply gouged. The latter two bones should not be excised below the insertions of the brachialis anticus and biceps muscles; for thus the brachial artery will be protected by the intervening brachialis muscle from any fair risk of injury in removing these portions of bone. Any surrounding *outgrowth* of bone, resulting from exuberant reparative action, must not be taken for disease; it should not be included in the excision. The line of excision having been closed with sutures, the arm is laid semiflexed on an angular splint; or it may be placed in Mr. Christopher Heath's splint—an apparatus which combines with rest and graduated flexion or extension, graduated elongation of the limb, and entire access to the joint for dressing. To ensure the formation of flexible union, passive motion should be gradually recommenced when such reparation is established; say, in three or four weeks.

Results in relation to life, or the mortality.—Three *general* conclusions may be drawn from the results of one large collection of cases, with those in the hospitals of the United Kingdom, and in the practice of individual surgeons. 1. A lower mortality than that of any joint-excision in the lower extremity; and in the upper extremity also, so far as statistics hitherto collected supply the data for comparison. 2. An average mortality of 1 in 8, or 12 per cent., or even much lower, about 1 in 15, or about 7 per cent. 3. A very different mortality in the hands of individual surgeons.

The conditions of disease, local and constitutional, in the cases selected for operation, would seem to mainly determine this difference; while the mode of performing the operation

and the after-treatment would also contribute to explain it. —*Lancet, Aug.* 26, 1871, *p.* 283.

37.—COMPOUND DISLOCATION OF THE RIGHT ELBOW—AMPUTATION OF THE ARM—NEW METHOD OF CONTROLLING HEMORRHAGE.

By THOMAS BRYANT, Esq., Surgeon, to Guy's Hospital.

An old woman, aged 77, who had fallen down upon her right hand on some stone steps, was admitted under the care of Mr. Bryant, at Guy's Hospital, with a compound dislocation of the elbow-joint. On examination, it was found that the ulna was displaced backwards without fracture of the coronoid process, a portion of the inner condyle of the humerus was detached, and there was a lacerated wound along the inner side of the biceps tendon. From the nature and extent of the injury, associated with the age of the patient, Mr. Bryant was induced to amputate at once. This he did by having the arm raised so as to empty the vessels of the limb, by gravitation, as much as possible of blood, and then, without applying the tourniquet, he controlled the brachial artery with the fingers of his left hand while he amputated just below the middle of the arm. The patient is now doing well.

On examining the joint after it had been removed from the body, it was seen that, besides the injuries alluded to, the head of the radius was comminuted and the upper end of the lower fragment or shaft of the bone dislocated forwards, the uneven end of which having been pushed inwards as well as forwards, had, no doubt, at the time of the accident produced the wound in the skin and soft parts on the inner side of the biceps. Instead of securing the artery by ligature or by torsion, which is the method chiefly employed at this Hospital, Mr. Bryant used an instrument which had been brought to his notice by Dr. Fleet Speirs, of New York. This instrument is called the artery constrictor, and consists of a flattened metal tube, six inches (more or less) in length, open at both ends, with a sliding steel tongue running its whole length, and having a vice arrangement at one end and a hook-shaped depression at the other, by which the artery can be grasped. It is then made to contract upon the artery by means of the vice at the upper end, which forces it within the sheath. In principle its action is the same as that of torsion, but the integrity of the external coat is more thoroughly preserved, while the continuity of the vessel is maintained.

Mr. Bryant informs us he had made some satisfactory experiments with the constrictor previous to using it upon the case

now described, and he is of opinion it will be of good service in cases where torsion is impracticable—as, for instance, in the cure of aneurism.—*Medical Times and Gazette, August* 12, 1871, *p.* 188.

38.—ON EXCISION OF THE WRIST.

By FREDERICK JAMES GANT, Esq., Surgeon to the Royal Free Hospital.

Excision of the wrist comprises, properly speaking, not only the removal of the articulatory portions forming the radio-carpal articulation or wrist-joint, but also the carpus and bases of the metacarpal bones; this extent of excision differing from that of the analogous operation on the ankle-joint, which is restricted to the removal of the articulatory portions of the tibia and fibula, with that of the astragalus.

Conditions of wrist-disease appropriate for excision.—Partial or *complete* disease of the wrist cannot be referred, like that of the larger and more simple joints, to an independent origin, either in the synovial membranes—three in number—or in the bones of the wrist. The lower articular end of the radius and that of the ulna, which in relation to excision is associated with the wrist—the carpus below, consisting of eight bones, in two ranges of four in each—and the bases of the five metacarpal bones, are all so contiguous as to obscure the precise seat of origin in caries affecting the wrist. Then, again, the three synovial membranes are as one, in relation to the origin of disease in the form of synovitis: the membranous investment of the radio-carpal articulation, which sometimes communicates with that of the radio-ulnar articulation through a perforation in the intervening triangular fibro-cartilage; the investment between the two ranges of carpal bones, with its two prolongations upwards, and sometimes extending into the synovial membrane of the radio-carpal articulation, and the three prolongations downwards which always extend to and invest the four inner carpo-metacarpal articulations; and the separate synovial sac for this articulation in the thumb.

Caries of the wrist appears to be generally of scrofulous, and thence constitutional, origin—excited, perhaps, by some injury of apparently trifling character, as a sprain; while synovitis seems to have another constitutional origin, probably as chronic rheumatism.

Thus we recognise as conditions of disease for excision:—

(1) Scrofulous caries of the wrist, often involving the lower articular end of the radius and ulna, the whole of the carpal bones, and the bases of the metacarpal bones, in a state of extensive caries.

(2) Chronic synovitis, of perhaps more limited extent, but leading to caries and destruction of the articulations.

The *amount of bone* to be removed, as being apparently diseased, may vary according to the character and extent of the disease. Thus excision might include the lower ends of the radius and ulna, with the adjoining carpal bones—in the wrist-joint, or the bases of some or all of the metacarpal bones. But Professor Lister insists on the complete extirpation of the wrist in *all* cases, from the lower ends of the radius and ulna to the bases of the five metacarpal bones inclusive; the disease, however limited it may appear, being apt to recur in the articulatory portions left by a partial excision.

Operation.—Excision of the wrist was originally performed by the younger Moreau, at the close of the last century; subsequently, by a German surgeon, Dietz, in 1839; and then again by Heyfelder, of Erlangen, in 1849; but, in this country, the operation was revived by Sir Wm. Fergusson, in August, 1851. Since that period it has been resorted to by Mr. Simon, Mr. Erichsen, the late Mr. Stanley, and Mr. Butcher of Dublin; and practised especially by Professor Lister of Edinburgh, who has devised a particular method of operation for complete excision of the wrist.

Partial excision consists in the removal of only one or two of the carpal bones, or other limited portions of the bones forming the wrist. This procedure can be readily effected by slitting up any fistulous aperture leading to the carious bone, and extracting it by bone-nippers and forceps.

Complete excision may be performed in either of three ways. The choice of method is mainly determined by the consideration of difficulty in removing the affected bones without dividing the extensor tendons of the fingers and thumb; the supinator tendons, radial and ulnar extensor tendons, inserted into the bases of metacarpal bones, being comparatively unimportant, in consequence of the firm fibrous anchylosis of the wrist after operation, if the result be successful.

First method.—A curvilinear incision is made, extending from just above the styloid process of the radius, downwards across the back of the wrist, and upwards to the same level above the styloid process of the ulna; the flap of integument is reflected, carefully avoiding the extensor tendons of the fingers, and those of the thumb, on the ulnar half of the radius. Then, dividing the supinator tendons, and the extensor tendons of the carpus, and flexing the wrist, the radio-carpal articulation is opened; and while the other extensor tendons referred to are drawn aside with a curved spatula by an assistant, the articular ends of the radius and ulna, the carpal bones, and bases of the metacarpus, are successively removed by a small saw or cutting pliers introduced transversely.

Second method.—Two *lateral* longitudinal incisions are made, one on the ulnar, the other on the radial side, of the wrist, or on its *dorsal* aspect, thus readily avoiding the extensor tendons of the fingers and that of the first joint of the thumb. The operation is then continued as before, and completed by excising the bones in the same manner.

A *single ulnar* incision is deemed sufficient by Sir W. Fergusson, and as the best mode of operation.

But Prof. Lister has particularly pointed out that there are two obvious and important objections to the lateral method, which somewhat resembles that which he has devised. First, the radial incision is so placed as probably to sacrifice the extensor tendons of the metacarpal bone, and of the second joint of the thumb. Secondly, with regard to the bones, that in the transverse division of the bones an unnecessarily large amount of bone is removed from the radius and ulna, and from the metacarpus—a loss of length and breadth which interrupts the process of consolidation, and results in a more narrow wrist and impaired strength of the hand. Moreover, the bones being divided in the dark, that some portion of the disease may probably be left behind.

To obviate these difficulties as to the excision of the bones, and to avoid the tendons requisite for the efficient use of the hand and fingers, another method of operation has been proposed and practised by Professor Lister. It consists in two essential peculiarities; the radial incision is so placed, on the dorsal aspect of the radius, as to avoid the tendons which are otherwise liable to be implicated—the extensor ossis metacarpi pollicis, and the extensor secundi internodii, while the limited but complete excision of the bones is accomplished by first removing the carpus, and then the articular ends of the radius and ulna and the bases of the five metacarpal bones. The hand is placed on a suitable splint, extending up the forearm and secured by a bandage. The most convenient form is an ordinary wooden splint, with an obtuse-angled piece of thick cork cemented to the palmar portion by means of fused gutta-percha, and with a bar of cork stuck on transversely to the under surface of the splint, so as to project at the side. Lister's splint, thus constructed, possesses certain important advantages. The hand lies semiflexed, which is its natural position of repose. The fingers are midway between flexion and extension, into which it is necessary to bring them by daily passive movements, while a certain range of voluntary motion is also permitted, which the patient should be encouraged to exercise frequently during the day. Then, again, this position is best adapted for allowing the extensors of the carpus to acquire fresh attachments; and, the palm resting on the sloping

surface of cork, the splint cannot slip upwards nor downwards, secured by turns of the bandage around the transverse bar of cork. This appendage to the splint specially keeps the *thumb* in position; it is thus allowed to fall below the level of the rest of the hand, so as to be most serviceable for opposition to the fingers, while the tendency to adduction of the thumb towards the index finger is prevented by a thick pad of lint placed in the angle between the two, under the turns of bandage around the transverse bar of cork. The palmar piece of cork should be hollowed out to receive the ball of the thumb.

After-treatment.—Two principles must be kept in view, in order to obtain a successful result. Firstly, to procure firm anchylosis of the wrist by retaining it in a fixed position, during the process of consolidation, for a period averaging six or seven weeks. Secondly, at the same time, to maintain the flexibility of the fingers and thumb daily; commencing flexion on the second day, whether inflammation has subsided or not. In executing these movements, each finger should be both flexed and extended to the full degree, while the connected metacarpal bone is held quite steady, so as not to disturb the wrist. Both these principles are provided for by the peculiar construction of the splint; and Professor Lister attaches more importance to their fulfilment, in the after-treatment, than to his method of operation.

Pronation and supination, also, must not be long neglected; and as the new wrist acquires firmness, flexion and extension, abduction and adduction, should be occasionally encouraged. The period during which passive motion should be practised may be resolved into this rule: it must continue until the disposition to contract adhesions finally ceases—a few weeks or a few months.

When the patient leaves his bed, and carries his arm in a sling, the weight of the hand will make it gradually droop to the ulnar side—a tendency which is best counteracted by affixing two ledges of gutta-percha to the ulnar side of the splint, one to support the border of the hand, and the other to prevent any lateral shifting of the splint. As the hand acquires strength, more free play for the fingers should be allowed, by cutting away the splint up to the knuckles, leaving only the palm supported. Some support must be continued until the patient feels the wrist as strong without it as with it. Earlier disuse of this support would assuredly undo the work of previous management, and lead to an unsuccessful result. A year, or even two, of such finishing-off support may be required to gain the most useful hand.—*Lancet*, *Sept.* 2, 1871, *p.* 314.

39.—A FEW PRACTICAL OBSERVATIONS ON THE TREATMENT OF WOUNDS.

By Dr. John Swift Walker, of Hanley.

[The treatment recommended by Dr. Walker is a modification of the antiseptic plan.]

First, take a simple incised wound of the scalp; after shaving the surrounding margin insert a suture (if large, a *sine quâ non*). In this class, freely apply Richardson's styptic colloid, and over this a pad of carded wool. In a few days the application of the solution will be again required, and a fresh piece of wool.

For simple incised wounds Richardson's solution is a very valuable addition to our Materia Medica, but only to be used as collodion or plaster to hold the two surfaces of a wound in position. It promotes union by the first intention, and if it does not so unite, the wound is not so gaping nor has it so large a cicatrix.

The carded wool or marine lint.—I have tried several kinds, but none seem to fulfil the object for which it is used so well as that obtained from Messrs. Wood, of Manchester, made by Mr. Westhropp, of the Falcon Works, London. But more of its special use hereafter.

Take now an amputation of arm or leg. After the operation, my plan of treatment is as follows:—Sponge out the wound thoroughly with chloride of zinc ʒ ss to ℥ j of water, then let all applications be quite dry for the first two or three days; as soon as there is the least secretion of pus, take a Higginson's syringe, and wash out the flaps with a strong solution of Condy's fluid, say ℥ j to ℥ x of water; apply strapping, and cover the wound with a small piece of lint wetted with a lotion of ʒ ij carbolic acid, ʒ ij liq. potassæ to six ounces of water; cover with a piece of gutta-percha tissue, and continue until healed, surrounding the edges or flaps with a little pad of carded lint.

Take another every-day case—an ulcer of the leg. If it is a large flabby ulcer, secreting a large quantity of pus, nothing is so good for compressing the granulations and checking the secretion of pus as the marine wool; but after it has fulfilled its object, a change of treatment, by the application of either a little dry precipitated chalk, zinc ointment, or zinc lotion, will heal it in a few days with rest and bandaging.

The marine lint is a very excellent application for a sinus after the removal of necrosed bone, and facilitates their closing with very little secretion of pus. Its tarry smell seems, in my opinion, to disinfect all ulcerating surfaces, but the application must not be continued too long, as the wool seems to contract the granulations, if such a term may be used.

I do not altogether agree with Mr. Lister as to the germ theory, but he has done good by teaching us to wash all wounds under our care with some disinfectant at each dressing; and every practitioner who will adopt this plan of treatment will be pleased at their satisfactory progress: and yet not a mere washing—it must be a stream of fluid. Any stump, after amputation either of the leg, arm, or finger, granulates very fast under this mode of treatment.

In my opinion, all Mr. Lister's solutions of carbolic acid are too strong,—and a brade the cuticle surrounding the wound; it may be through my clumsy mode of application, or not following the minute directions laid down. Thus far I can admire his treatment, that it teaches us all that we disturb the approximating surfaces of wounds too often very unnecessarily, and thereby retard nature's efforts.

Since writing the above, my attention has been directed to a paper in the *Lancet* of the 13th May, on the use of a similar article, "Tenax." This I have not had as yet; but doubtless it produces the same therapeutical effects as the carded wool. Its chief advantages seem to arise from being so porous as to soak up all discharges, and the tar or ingredients of the tar, probably the creosote, so neutralises and changes the character of the discharge that it becomes quite innocuous: for we see the same effect in an injection of creosote in corroding ulcer of the uterus or in cancer. But a creosote lotion has not the same effect upon open wounds.

My chief motive in writing this paper is to direct the attention of my professional brethren to the use of some preparation as the carded wool, as its therapeutical effects are only known to a few, and these highly appreciate its use.—*Practitioner, Oct.* 1871, *p.* 210.

40.—M. GUERIN'S NEW METHOD OF DRESSING WOUNDS.

During the siege, the Paris surgeons were much grieved and disappointed at witnessing the utter failure which attended the attempts at amputation and disarticulation, or the liberal use of the knife in any manner. In such cases a fatal result was almost sure to ensue, whatever the mode of dressing employed; and the surgeon was at a loss to discover what means to resort to for saving the patient's life. Attempts at conservative surgery were crowned with much comparative success; still the general results were very unsatisfactory, and the wits and abilities of the Paris surgeons were stretched to the utmost in their search after some safe mode of proceeding. It was quite obvious that the unsatisfactory character of all general hygienic conditions—the want of proper food, the moral state

of the patients, depressed and disturbed by the events and emotions of the time, and the bad condition of the atmosphere—combined to entail this heavy mortality. The character of the wounds was also far more serious than had ever been observed under like circumstances; and the injuries inflicted by the cone-shaped bullet (tearing up the soft parts, splintering the bone in all directions, and causing extensive contusion of the marrow) necessarily brought on most disastrous consequences. The results of private practice were scarcely more favourable than those obtained in the wards of the hospitals; and this was observed to be the case as much during the reign of the Commune as during the German seige.

Towards the end of the Communal *régime* M. Alphonse Guérin, who had been previously much struck by the statements advanced by Pasteur, Tyndall, and others, in regard to the dust and germ theory of disease, bethought him of applying a new mode of dressings founded on these doctrines. The starting-point of his inferences and experiments was simply the filtration of dust and germs through cotton, as illustrated by Tyndall's experiments. His first attempts were attended by moderately favourable results, but he has since completed and perfected his system with a success which deserves the attention of surgeons.

The *modus operandi* is extremely simple. It consists, if we take the dressing of an amputation for example, in wrapping the stump round and round with successive layers of cotton. A liberal use of the substance must be made, and several yards of cotton wool successively disappear around the limb. It is quite obvious—to speak in the words of the surgeon of the Hôpital St. Louis—that this thick interposition of clothing is requisite to filter the air before it reaches the wound. It is not the less necessary to extend this dressing in all the directions of the limb, as foul or unfiltered air might find its way to the injured part. Thus, in amputation of the thigh, thick layers of cotton-wool are carried up to the hip and round the waist and the nates, so that all the approaches to the wound are carefully guarded in every direction. Coupled with this, M. Guérin exerts a gentle pressure over all the enclosed parts by means of ordinary cotton bands—a point to which he attaches great importance, as it enables him to tighten the whole appliance and to keep the parts snugly together.

This constitutes the entire proceeding. It may be observed that M. Guérin does not use carbolic acid in any way whatever. The wound is simply washed with camphorated alcohol after the operation. The surgeon's hands, the sponges, and instruments undergo no kind of preparation before the operation. The cotton-wool and cotton bands are steeped in no fluid.

M. Guérin lays great stress on the importance of carefully watching this dressing, which it is interesting to observe is intended to be a permanent one. He watches the dressing day by day, and never takes it off unless some extraordinary circumstance occurs, but contents himself with adding fresh layers of cotton-wool if he observes that it is in any way disturbed in such a manner as to permit of the introduction of unfiltered air into the wound. Thus the dressing may remain *in situ*, and does so in the great majority of cases, for twenty-five or thirty days. On removing the dressing after this lapse of time a healthy granulating surface is discovered, and half a wine-glassful of healthy pus is found within the folds of the cotton. It may here be stated that M. Guérin, on applying a first dressing, stuffs up the stump with cotton-wool, which he introduces beneath the flaps. The process of granulation gradually drives out the cotton, and cicatrisation takes place perfectly. Moreover, this mode of dressing may be applied, and has been with great success, to extensively abraded and burnt surfaces.

Such are the details of M. Guérin's proceeding; and the results which have attended it are deserving of much attention, if we consider the mortality which habitually follows such operations in Paris, and which was especially formidable during the two sieges, and if we also take into account the fact that almost all M. Guérin's operations were for gunshot injuries.—*Lancet, Sept.* 2, 1871, *p.* 322.

41.—MICHEL'S PROCESS FOR REMOVING EXTERNAL TUMOURS.

By Dr. William A. Bell, M.A., Cantab.

A little pamphlet gives an interesting account of the mode of operation for the removal of tumours practised by a French charlatan, for a knowledge of which Dr. Bell paid no less a sum than 25,000 francs, and which, having now obtained complete information, he has very properly and liberally made public. The preparation used in all cases where the tumour can with safety be reached externally is made in the following way. Asbestos, as soft and free from grit as possible, is reduced by rubbing between the hands to the finest possible fleecy powder; it is then mixed thoroughly with three times its own weight of strong sulphuric acid ($S\,O_3\,H\,O$). A mass is thus formed which may be easily worked with a silver or gold spatula into any size or shape, corresponding to the tumour to be destroyed. Any malignant growth of the breast which is detached and solitary, with the subaxillary glands unaffected, is suitable for treatment, whether open or not makes no differ-

ence. In the application of the caustic the adjoining healthy parts of the skin are carefully protected by applying a zone of collodion and pads of linen, and the patient is so placed that the surface of the tumour is perfectly level. The saturated acid asbestos is then laid on the surface to the thickness of half an inch for a tumour the size of a hen's egg. Rapid destruction of the tissues follows, with, after the first half-hour or so, but little pain. An oozing of clear watery fluid appears, which must be carefully sopped up. After twelve or fourteen hours' action, the first application is to be removed, and a new portion of smaller size adapted to the sore. After this has been applied for twelve hours, the operation is complete, and the healing of the deep excavation alone requires to be attended to, for the details of which we must refer our readers to the pamphlet. Dr. Bell does not pretend to say that this mode of operation will effect a permanent cure of cancerous cases, but he thinks that the plan presents various and considerable advantages over extirpation by the knife, as in producing much less shock to the system, in removing the tumour alone with but little of the surrounding breast, and in postponing, in malignant cases, for a longer period the recurrence of the disease.—*Practitioner, June* 1871, *p.* 377.

42.—ON A PECULIAR FORM OF WRIST-DROP FROM PARALYSIS OF THE MUSCULO-SPIRAL NERVE IN FRACTURE OF THE HUMERUS.

By Professor ERICHSEN, Senior Surgeon to University College Hospital, and Holme Professor of Clinical Surgery.

Simple fractures of the long bones are seldom accompanied by any serious complications; the vessels and nerves of the limbs being so situated, and being so well protected by the interposition of a layer of muscle between them and the bones, as to escape being injured, in the vast majority of cases, by the ends of the fractured fragments. There are, however, two exceptions to this general rule: one in the upper, the other in the lower extremity. In the upper extremity, in the relation of the musculo-spiral nerve to the shaft of the humerus; in the lower, in the position of the posterior tibial artery in reference to the upper end of the tibia. In both these instances the position of the nerve and of the vessel is such that it may be seriously injured by fracture of the contiguous long bone.

It is not my intention to speak of the injuries that the posterior tibial artery may sustain in and from simple fractures of the bones of the leg; but I shall confine my observations

wholly to lesions of the musculo-spiral nerve from fractures of the humerus.

The complication of an injury of the musculo-spiral nerve, in a case of simple fracture of the humerus, must certainly be of rare occurrence, as I find no mention made of it in the standard works on surgery that I have had an opportunity of consulting, and I do not recollect to have met with this accident until recently. We have, however, had lately three cases under our care here, in which, in consequence of injury to the musculo-spiral nerve, or one of its branches, more or less complete paralysis of the muscles supplied by that nerve has resulted. These cases I will presently relate to you, and on them I will found the observations I have to make on this interesting subject.

When we observe the manner in which the musculo-spiral nerve winds closely round the back of the shaft of the humerus in its flattened groove, and how in its course downwards towards the forearm it comes into tolerably close relations to the outer condyle, we can easily understand how, in fractures of the shaft, the main trunk may be implicated, and in those of the condyle one or other of its chief divisions injured.

When the main trunk of the musculo-spiral nerve is injured to such an extent as to induce complete paralysis of it, both the supinators of the forearm, and all the extensors of the wrist and fingers lose their power, and the patient becomes utterly incapable of performing those movements that are dependent on the action of these muscles. The hand consequently falls into a state of pronation and flexion, presenting the characteristic signs of "wrist-drop."

When the posterior interosseous division of the musculo-spiral is the nerve that is injured, the loss of supination and of extension is not so complete. The supinator longus and extensor carpi radialis longior, being supplied by branches from the main trunk, are not paralysed, and thus a certain, though very limited movement, in the sense of supination and extension is preserved, although the forearm and hand fall naturally into a state of pronation and flexion.

These different conditions were well illustrated in the following three cases, which I will relate to you in the order of their degrees of extent and severity, so far as regards the paralysis of the various sets of muscles. An attentive study of the movements of the wrist, hand, and fingers, in these cases, throws a clear light on the actions of the muscles supplied by the musculo-spiral nerve and its branches, and some of the other nerves of the forearm and hand.

Case 1.—Fracture of Shaft of Humerus; paralysis of trunk of musculo-spiral nerve, and complete loss of extension of the wrist,

fingers, and thumb; loss, nearly complete, of supination.—E. L., aged 29, by occupation an ironer, admitted Dec. 16th. Ten weeks since the patient fell and fractured her humerus about the middle. She was treated as an out-patient, and the limb put upon an angular splint, so as to fix the elbow-joint. When the splint was left off, at the end of four weeks, she was directed not to use her arm for a week. She noticed wrist-drop when she left off the splint, but thought it was mere weakness. At the end of a week, however, on trying to use her hand, she found that she had no power in the wrist or fingers. It was thought to be weakness, and she bathed it with cold water. The hand has for some time felt much colder than the other. On examination, it is found that there is a marked wrist-drop, with pronation. She cannot extend the hand at all. The right forearm, hand, and fingers are swollen. She feels a difference in the temperature of the two hands, but not so great as formerly. Occasionally she has a feeling of pins and needles all down the hands and fingers. The thumb and index-finger are numb on the dorsal aspect, especially the former. Sensation over them is imperfect. Flexion of the fingers is imperfect owing to stiffness of the knuckles. The temperature of the affected hand does not raise the index of clinical thermometer to 85°; that of the other is 90·6°. All the muscles supplied by the musculo-spiral nerve are paralysed completely. She has consequently totally lost all power of extending the wrist. She has no power of extending the thumb. She has no power of extending the fingers from the metacarpo-phalangeal joints; but when the fingers are completely flexed she can extend the joints between the first and second and second and third phalanges. This is evidently accomplished by means of the interossei and lumbricales, which are attached to the expansion of the extensor tendons on the dorsum of the fingers lower down than the metacarpo-phalangeal joints. On holding the index-finger forcibly down, and telling her to try to extend it, the thumb is drawn in towards the palm by the attachment of the first dorsal interosseous to the metacarpal bone of the thumb. Supination can be performed apparently in a very feeble and imperfect manner, but only when the forearm is flexed. Flexion of wrist, hand, and fingers is perfect.

Since this report she has been galvanised by faradisation regularly, and has regained considerable power in all the affected muscles, so that she can now extend the wrist and the fingers from their metacarpo-phalangeal articulations.

There are several points in this case that deserve special attention. That the trunk of the musculo-spiral nerve was paralysed by being implicated in the fracture which occurred in that part of the humerus round which it winds, there can be no

doubt. All the muscles connected with the hand and wrist that are supplied by both of the terminal divisions of that nerve —the radial and the posterior interosseous—were paralysed, and none other were affected.

Now let us examine a little more in detail the conditions of the hand and fingers. The wrist-joint was flexed, so that the hand hung listless and inactive, at nearly right angles with the forearm. It could not be raised or extended in the slightest degree. No effort that the patient made in this direction was of any avail. There was, consequently, complete paralysis of the two extensors of the wrist—the long and the short. The hand was prone; complete supination was impracticable, and no movement whatever in that direction could take place when the forearm was extended on the arm. But when the forearm was flexed, a slight supine movement could be made by the patient. To what was this due? Clearly not to the supinators, which would have acted equally well whatever the position of the elbow might have been. But apparently the slight effort at supination, for it was really nothing more, was the result of contraction of the biceps, which, as you are aware, when called into action whilst the hand is prone, has for its first effect a tendency to supinate the forearm and hand. There was, consequently, complete paralysis of the two true supinators—the long and the short. Thus there must have been loss of innervation in both the terminal branches of the musculo-spiral—the radial and the posterior interosseous. The long extensor of the fingers was paralysed, so that when they hung at right angles with the hand, they could not possibly be extended from the metacarpo-phalangeal articulations so as to be brought to a level with the dorsum of the hand. But there was one movement of extension still left to the fingers, and it was this—that when they were bent or drawn into the palm of the hand, the last two phalanges could be extended, and with some little force, from the articulations between the first and second phalanges. Now this is an extremely interesting point, and one to which I would direct your close attention. To what is this upward or extending movement of the two terminal phalanges due?—a movement that takes place independently of the action of the true extensors of the fingers. It is due to the combined action of the interossei and lumbricales—muscles that do not receive their innervation from the musculo-spiral, but chiefly from the deep branch of the ulnar; all the interossei and the two innermost lumbricales receiving their nerves from this source, whilst the two outermost of the lumbricales obtain theirs from the median. Thus these accessory muscles of extension, receiving their nervous supply from sources that were uninjured, continued in the free exercise of their special actions.

The thumb was drawn in towards the palm, and could not be abducted owing to the paralysis of the extensor muscle of the metacarpal bone. The numbness and referred sensations occupied those portions of integument that were supplied by the terminal branches of the radial nerve. The temperature of the hand was considerably lower than the other. It was below the lowest mark (85°F.) on the clinical thermometer; and as that of the sound hand was 90·6°, there must have been a difference of at least 5° to 6° F. between the two limbs.

The next case that I will relate to you is one in which the paralysis seems to have been limited to the posterior interosseous nerve. It is as follows:—

Case 2.—Wrist-drop following Compound Fracture of the External Condyle of the Humerus; paralysis confined to the muscles supplied by the posterior interosseous nerve.—H. E., aged 30, by occupation a lace-cleaner, on the 17th of November slipped down on her right elbow on the pavement, and afterwards came to the hospital. On examining the elbow, much mobility and crepitus were found, and a fracture of both condyles, with separation, could be clearly made out. At the back of the arm, about an inch above the elbow, was a wound which would take the tip of the little finger, clean cut without bruising of the edges, apparently done by protrusion of bone at the time of the fall. A probe could pass in different directions readily among the tissues. From Nov. 17th to Dec. 21st the limb was kept on an angular splint at the inner side, and the wound treated in strict accordance with Lister's rules for antiseptic dressing. On Nov. 24th (eighth day) there was much swelling, with some redness and tension, about the joint and upper half of the forearm. It was thought that fluctuation existed; and an incision, about three-quarters of an inch long, was made a little below the elbow. Much serous fluid came, but no pus. Large quantities of serous fluid continued to come from the wound for a fortnight afterwards. The tension rapidly disappeared; no pus was at any time observed. Passive motion was commenced on Dec. 21st. The splint was left off on Dec. 23rd. There was much stiffness of the joint.

Jan. 23rd. Patient can bend the elbow to an angle of 45°; can straighten to about a right angle and a half. Can close the hand as far as to bring the tips of the fingers to about an inch from the palm; her hand can be made to close completely without much difficulty. There is perfect flexing and opposing power in the thumb. When the hand and forearm are supinated the wrist is quite straight. Cannot completely straighten the hand at the metacarpo-phalangeal joint. Very slight force brings these joints straight; but she cannot extend the fingers. When the hand and forearm are pronated there is a complete

drop of the wrist. Cannot raise wrist or fingers. Cannot move the thumb outwards or backwards (through loss of power of the extensor.) There is almost perfect power of supination when the elbow is fixed. When the fingers are completely flexed, she is able to extend the joints between the second and third and first and second phalanges, by means of the lumbricales and interossei. When doing so, as the hand is very thin, the interossei can be seen working. There is slight numbness on the back of the thumb and index finger, but no absolute loss of sensation.

Now this case closely resembled the last in all respects, with the sole exception of the paralysis of the hand and arm not being so complete. The general aspect of the limb was the same. The pronation and flexion of the forearm and wrist were marked; but the power of supination was not completely lost—in fact, existed to a considerable degree, but was not perfect. So also with regard to extension of the hand from the wrist. The knuckles could be brought up nearly to their proper level. Now this imperfect power of supination and of extension of the hand was doubtless due to the supinator longus and extensor carpi radialis longior—muscles supplied by the radial nerve—retaining their power, and thus being able to act; whilst the short supinator and the short extensor of the wrist, both supplied by the posterior interosseous nerve, were completely paralysed. Hence the imperfection of the supination and extension that existed. There was further proof of the fact of the radial nerve having continued to maintain its action in the fact that sensation was not lost in its terminal cutaneous branches. The temperature of the hand also had not fallen, as in the first case.

Case 3.—*Fracture of Lower Epiphysis of Humerus; wrist-drop from paralysis of posterior interosseous nerve; tonic contraction of flexors.*—M. M., aged seven, was admitted as an out-patient under Mr. Heath, and by him transferred to Mr. Erichsen. In June, 1870, she fell over a croquet hoop, and the lower part of the right humerus was fractured. The arm was at first supposed by her friends to be dislocated, and a non-medical gentleman who was present pulled violently at it for some time, but, as he did no good, she was taken to a medical man. Splints were used for seven weeks; they reached to the tips of the fingers, which were kept extended. When the splints were left off the fingers became flexed at once. Her parents think they were more so than now. She can crotchet with the right hand, and can write, but badly; she has been learning to write with the left hand in consequence. She has very marked wrist-drop; she can, however, easily extend her wrist. Her hand is pronated, and can only be imperfectly supinated. The fingers

are flexed and drawn into the palm of the hand. On the wrist being dropped, the last two phalanges of the fingers can be imperfectly extended by the patient. On the wrist being straightened, the fingers become flexed, and cannot be extended actively or passively. On forcibly extending the fingers and wrist, there is no tension of the palmar fascia, but there is great tension of the flexor tendons above the wrist. The hand is congested and cold. The arm is distinctly smaller than the other. The temperature of the right palm is not high enough to move the index of the thermometer—so that it is below 85°, that of the left being 93·6°. The sensibility of the hands was tested by the compasses; it seems quite as acute in the right as the left. There is some irregularity of the lower end of the outer condyle, which seems to have been the situation of the fracture, or separation of the epiphysis, which was the original injury.

Dec. 19th. Mr. Erichsen ordered a splint to be specially constructed so as to permit of gradual extension of fingers by means of a moveable hand-piece worked by a rack and pinion.

Jan. 4th, 1871. Mr. Erichsen ordered her arm to be faradised daily. There was at first scarcely any contractility perceptible in the extensors.

9th. The splint has been applied, and the arm has been galvanised daily. The contractility of the extensors and supinators has markedly increased.

23rd. The fingers have become sore from the pressure of the instrument. The index finger is but little improved. The middle finger is better, and the little and ring fingers are very greatly so, being now almost straight. The wrist can be extended perfectly, so that the knuckles can be brought to a level with the back of the forearm. When so extended, the fingers are half bent. But when the wrist is dropped they can be extended by the patient. In doing this they always involuntarily spread out in a fanshape, owing to the action of the dorsal interossei.

It would appear that the chief resistance to proper extension was due to the contraction of the flexor carpi radialis and the flexor tendon of the index finger, both of which are very tense. Mr. Erichsen proposed to divide these subcutaneously; but the child's friends would not give their consent, and the patient was consequently discharged from the hospital.

In this case, also, we had the partial loss of supination and of extension dependent on paralysis of the posterior interosseous nerve; whilst those movements that were due to the interossei and lumbricales were perfect. The contraction of the flexors, which had become very marked, was apparently due to the loss of action of their antagonist muscles. It was most

marked in the flexors towards the radial aspect of the forearm, and was also associated with a tonic pronation of the limb. The muscles thus injuriously affecting its movement had apparently undergone some rigid atrophy; and I regret that the child's friends would not allow tenotomy, as it offered a good prospect of cure. The fall in the temperature of the hand was very marked. It amounted to at least $8\frac{1}{2}°$ F., and how much more it was impossible to say, owing to the marking of the thermometer not admitting of a wider range being noted. But the difference between the two hands in this respect was most obvious and very sensible to the touch. This great fall in the temperature of the hand is very remarkable when we reflect that it was due to paralysis of a branch of the musculo-spiral which is not directly distributed to the hand; whilst the other nerves of the hand—the median and ulnar and cutaneous branch—were intact, and gave no evidence of paralysis. The movements of the muscles of the hand itself, and the sensibility of the skin covering it, were normal.—*Lancet, July* 1, 1871, *p.* 1.

ORGANS OF CIRCULATION.

43.—ON ACUPRESSURE.

By William Pirrie, Esq., F.R.S.E., Professor of Surgery in the University of Aberdeen.

[The late Sir James Simpson possibly thought correctly when he said "the invention of acupressure will one day do more for my fame than even the discovery of chloroform." At his last interview with Sir James Simpson, Mr. Pirrie stated his belief to him that the more general adoption of acupressure would be greatly accelerated by its friends recommending *three* methods *only*, and by their agreeing to distinguish them not by the old numbers, which would be no longer applicable, but by a new nomenclature. Sir J. Y. Simpson highly approved of these proposals, and on the following day sent for consideration the names hereafter mentioned.]

Three modes only of acupressure are required, and it was resolved by the late Sir James Y. Simpson and myself to designate them by the names of Circumclusion, Torsoclusion, and Retroclusion. In arresting hemorrhage by circumclusion, an acupressure pin and a loop of inelastic iron wire are employed, whereas for the performance both of torsoclusion and retroclusion a pin only is required.

I prefer the pins suggested by myself to any others; and those who are desirous of using them will have no difficulty in obtaining them, and the proper kind of inelastic iron wire, from Messrs. Weiss, Strand, or from Messrs. Coxeter, Grafton-street East, London.

Circumclusion.—As the metal goes round the vessel, this mode of arresting hemorrhage is called circumclusion. It consists in entering a pin in the tissues a line or two to one side of the artery to be circumcluded; pushing it behind, and causing its point to emerge a little beyond the vessel; passing a loop of inelastic iron wire over the point of the pin, bringing the wire over the track of the artery and behind the stem of the pin, drawing it sufficiently tight to close the vessel, which is proved by the arrest of bleeding, and fixing it by a single twist around the pin. This description will make the mode of performing circumclusion sufficiently clear, and the *rationale* of its operation in arresting hemorrhage perfectly intelligible.

In my experience of acupressure, no fact has struck me more than the slight degree of pressure required for the perfect arrest of circulation through an artery, provided the pressure be direct and continuous. In performing circumclusion, it is important to have this fact impressed upon the mind, so that unnecessary and injurious constriction may be avoided. Of all methods of acupressure this is the best. It is perfectly reliable, it is most generally applicable, it may be performed in an amazingly short time, and, like the other methods in ordinary circumstances, without the aid of any assistant. When an artery is situated in a confined corner of a wound, or when cut short in a perpendicular surface, by taking hold of the mouth of the vessel with an artery forceps, and drawing it out very gently, the passing of the pin below, and the loop above, the artery will be greatly facilitated. When the artery is to be relieved from acupressure, the head of the pin should be gently twirled, and, having thus been rendered loose, may be pulled out with the greatest ease without causing any pain; after which the loop, being liberated, can be readily withdrawn.

Torsoclusion.—Torsoclusion, formerly described by me under the name of the Aberdeen method, or the method by the twist, requires for its performance a pin only, and consists of two steps.

In the first step, the pin is inserted in the tissues pretty close to the mouth of the bleeding artery; it is then pressed onwards in a direction parallel to that of the vessel; and its point is caused to emerge to the extent of several lines.

The second step consists in giving a quarter rotation to the pin, so as to place its emerged extremity above and at right angles to the artery; in pressing it well down against the small

portion of tissue between the instrument and the vessel; and in sending the point for some distance into the tissues beyond the artery, for the purpose of securing the pin in position and of maintaining the twist. The artery is to a certain extent twisted, as well as steadily compressed, and in that way, no doubt, the occlusion of the vessel and the suppresion of the hemorrhage are effected.

Of all methods of acupressure, that by torsoclusion as now described is the simplest, the easiest, the quickest, and it has the indispensable recommendation of being perfectly efficient. In reference to the advantages of this method, I beg to be allowed to quote from my own writings. "Simplicity, efficiency, quickness, and ease of performance, are unquestionably great recommendations; but this method possesses two other advantages, of the greatest importance for obtaining either immediate union or union by primary adhesion: the one, that there can be but little molecular injury or straining of tissues; and the other, that, by the gentlest twirl and traction, the pin can be easily withdrawn, with extremely little, if any, discomfort to the patient. Surgeons who know how little produces pain in an amputation wound, how slight a degree of pain is apt to cause involuntary contraction of muscles in the stump, how certain such contractions are to separate parts of the internal surfaces of the wound from each other, although by retentive means its edges are kept in apposition, and who, after the greatest anxiety to obtain the best results, have so often, in a few days, witnessed the disheartening effects of such contractions, will fully appreciate the value of a means of arresting hemorrhage which, at the moment deemed judicious, can be removed, not only without pain, but almost without discomfort to the patient.

The first time the femoral artery was secured by this method was by myself, in an amputation of the thigh, in the Abeerdeen Hospital, in June, 1864; when those present were exceedingly gratified with the great simplicity of the method, as well as the facility with which it could be employed. Since that time it has been employed in an immense number of cases, by the late Dr. Keith, by Dr. Fiddes, and myself; and it is to be hoped that our united testimony in favour of this admirable mode of arresting hemorrhage may induce surgeons of other hospitals to give it a fair trial.

Retroclusion—so named in consequence of the pin passing ultimately behind the artery—is a convenient mode of securing vessels of small or very moderate size, and, like torsoclusion, consists of two steps, and, like that method of acupressure, requires a pin only for its performance. In the first step, the pin is entered in the muscular tissue, a little to one side of the

artery, held almost flat upon the wound, caused to emerge, and passed in front and a little beyond the track of the vessel. It should be particularly observed that, in this movement, only a few muscular fasiculi are raised upon the pin, and that it is caused to emerge from the tissues before being sent across the track of the artery.

In the second step, the head of the pin is made to describe the greater part of a semicircle, so as to be placed nearly flat on the opposite side of the wound; its point is then sent behind the artery, in the contrary direction to that in which it passed in the first movement, and pressed on until it is fixed in the tissues beyond the vessel.

When is it safe to free an artery from Acupressure?—If one could tell the earliest time at which an artery could be safely freed from acupressure, that would accelerate its more general adoption; but time and experience alone can teach this, and time and experience have not yet done that work. Nevertheless, they have done enough to teach us that, in ordinary circumstances, where acupressure has been skilfully performed, vessels of comparatively small size, such as the facial, temporal, radial, ulnar, mammary, and spermatic, may be safely freed from acupressure in eight hours; and larger arteries, as the humeral, axillary, and femoral, in twenty-four. I have a strong impression that future experience will show that the early friends of acupressure have been too cautious in removing the compression, thereby losing the full advantages of its use, and that much shorter time will yet be proved to be sufficient; and the shorter the better for increasing our chance of the most coveted methods of healing wounds. On the adoption of acupressure in Aberdeen, in 1864, we had no experience to guide us on this most interesting point, forty-eight to seventy-two hours being then deemed necessary; and, in working out this problem, we have perhaps gone on too slowly shortening the time; but we felt ourselves bound not to expose our patients to untried risks, and also not to injure what we consider a valuable proceeding by the occurrence of hemorrhage from imprudent haste.

Two cases produced a strong impression on my mind, as holding out great encouragement to shorten the duration of acupressure. One was that of a little boy, whose thigh was amputated by the late Dr. Keith, and who, four hours after operation, in the absence of the nurse, took out the pin which compressed the femoral artery without any bad result. The other was that of a girl, whose arm I amputated, and fifteen hours afterwards relieved the axillary artery, without the withdrawal of the pin having been followed by a drop of blood.

Every one knows how greatly the dread of hemorrhage has retarded the progress of surgery; but to show how little risk there is of bleeding on relieving arteries from acupressure, I may state that the late Dr. Keith and myself, in hospital and private practice, compressed altogether upwards of two thousand arteries; that in one only of Dr. Keith's cases was there bleeding on the removal of the pin, and in one only of my own; and in each case the artery was again acupressed in a few seconds. In the one case, Dr. Keith had performed amputation in the forearm, and the bleeding was from the radial; in the other, I had performed amputation of the upper third of the leg, and the hemorrhage was from the anterior tibial artery. We read of the pin having been left for five days compressing the femoral artery, which is contrary to every rule that obtains in relation to the process; and the wonder is, that, left so long, it should not have caused irritating results.

Effects of Acupressure on Arteries.—On dissection, I have not seen any division or laceration, any discoverable injury or degeneration, any suppuration or sloughing of the coats of an artery at the site where it had been acupressed; and, with the exception of two cases, the condition has been occlusion from adhesion of the internal surface, and an internal coagulum more or less adherent to the artery. In one of the exceptional cases there was a coagulum an inch in length adherent to the artery, without adhesion of the internal surface. In the other, four vessels had been acupressed; in the two larger there was an internal coagulum and adhesion at the site of the compression; and the two smaller were closed by lymph at their cut extremities without an internal coagulum. In the first-mentioned case, death was caused by shock thirty-six hours after operation; and in the second, by violent dysentery, which commenced on the third, and proved fatal on the tenth day after operation.

Merits of Acupressure.—Under this head there are two questions to which, as a firm believer in the advantages of acupressure, I earnestly beg the unprejudiced consideration of surgeons.

1st. *Is Acupressure a practicable and trustworthy method of arresting surgical hemorrhage?* That it is so is incontrovertibly proved by the fact that, since March, 1864, with the exception of two cases to be afterwards mentioned, in all my operations, both in hospital and private practice, comprehending, among many others, amputations in the thigh, leg, arm, and forearm, and at the ankle-joints; in excision of the mamma, testicle, knee and elbow-joint, those of the mamma and elbow-joint having been very numerous; in all operations admitting of the

surfaces of the wound being brought together—in short, in almost every variety of operation, age, and sex, I have invariably employed acupressure, and never in a single instance failed in arresting the hemorrhage. What stronger proof of the practicability and trustworthiness of acupressure could possibly be desired? If to this evidence be added the experience of the late Dr. Keith, and that of Dr. Fiddes, two unwavering believers in the superiority of acupressure over every other method of arresting surgical hemorrhage, whose cases, along with my own, constitute the experience of acupressure at the Aberdeen Infirmary, surely the question of the practicability and trustworthiness of this method of checking surgical hemorrhage must be set at rest.

In two important operations above referred to—the one the removal of a tumour upon and underneath the angle of the jaw, the other excision of the lower jaw, submaxillary and sublingual glands, and a portion of the tongue—I considered it prudent to give the preference to the ligature, as the pins during their brief sojourn in the wound might have caused inconvenience during deglutition.

2nd. *What are the special merits of Acupressure?* The greater tolerance of living tissues to metallic than to textile bodies; the brief sojourn of the hæmostatic agent in the wound; the satisfaction to the surgeon of being able to remove that agent the moment removal is deemed advisable; the comfort and encouragement experienced by the patient on being assured, at the conclusion of a very brief period after operation, that all foreign matter has been removed from the wound; the immunity of the coats of arteries from laceration and sloughing, and, in consequence of that immunity, the diminution of the risk of septic poisoning, a case of which I have not seen since the adoption of this method of arresting hemorrhage—are unquestionably great advantages of acupressure. The superiority of acupressure, however, will be made more apparent when we consider what results we desire to obtain after operation, and how these may be best secured.

Union by the first intention, or by primary adhesion, is the aim of every surgeon, where accurate coaptation of surfaces and edges can be maintained. So long as we leave foreign bodies in wounds this much to be desired result cannot be obtained. If, on the other hand, we free them from all such bodies at a period so early that their presence cannot give rise to irritation, an insuperable obstacle is removed, and union by one or other of these methods will in all probability ensue. Here, then, is demonstrated the superiority of acupressure over all other means yet devised for the arrest of surgical hemorrhage. For a few hours after an operation has been performed,

we can with perfect safety withdraw the pins, and all *local* hindrance to early union is thus overcome. My own experience, as well as that of the late Dr. Keith, and of Dr. Fiddes, proves the correctness of this statement. Prior to our adoption of acupressure, in 1864, I had never seen, in any case where a large vessel had been secured, a perfect example of union by the first intention. Since then I have had examples of this method of healing in every amputation of the body, with the exception of the leg and at the hip-joint—the latter operation I have not, during that period, had occasion to perform,—in excision of joints and tumours, and many other minor operations. In addition to cases of union by the first intention occurring in the practice of the late Dr. Keith, Dr. Fiddes, and myself, already published, let me mention one or two remarkable examples of early union.

On a girl twelve years of age, I performed, according to my own method, amputation at the ankle-joint, on account of extensive caries of the bones of the foot. Three vessels were acupressed; the wound was exposed to the air until its surfaces had become glazed, and its edges, having been brought into accurate apposition, were retained simply by strips of plaster. The wound healed without there having been a single drop of discharge of any kind; and on the twelfth day after operation, the patient, being perfectly convalescent, left the hospital. Such a result cannot but be considered unusual in an amputation of the ankle-joint.

One of the most striking cases I have ever seen was that of a female, the subject of a huge adenoid tumour of the mamma, which I excised, she being at the time in the ninth month of pregnancy. The tumour weighed 13½ lb. The wound healed entirely by the first intention; fifteen days after the operation she was discharged from the hospital, and on the ninth day following she gave birth to a healthy female child, which she was enabled to suckle, much to her own satisfaction, as she attributed the loss of two former children to the want of breast milk.

In another case—a private one—a carcinomatous mamma, of large size, was excised on a Friday, and on the following Sunday week the patient went to church, the wound having entirely healed by that day, without the formation of a single drop of pus.

In another case of the same disease, I removed a large mamma; perfect union by first intention took place; and fourteen days afterwards the patient left the hospital, remarking that she had enjoyed her residence there very much—rather an unusual statement for a patient to make who had been the subject of operation at a period so immediately preceding.

I might enumerate many other equally striking cases. but for the present the foregoing must suffice. The favourable results I have experienced during the last seven years I attribute entirely to the coats of the vessels secured having been neither cut nor lacerated, to the non-irritative properties and early removal of the hemostatic agent used, and to the comparative disuse of dressings. With regard to the last-mentioned point, I may state that, with the exception of wet lint, I do not now employ any dressings, nor do I use antiseptic substances.

Before bringing this paper to a conclusion, let me remark that acupressure is far from difficult of application, and that any slight difficulty that may be at first experienced will, with a little practice, disappear; and the results following its employment will repay a thousandfold any effort made for acquiring dexterity in its use.

I have now done my best, not in any controversial spirit, to express clearly and briefly my decided convictions regarding the essential modes and merits of acupressure; and as it is specially important, for the interests of those who may become the subject of surgical operation, that this method of treatment should be justly appreciated by the great body of surgeons, I earnestly hope that a fair and skilful trial—the only means by which a correct judgment can be arrived at—may be as fully accorded to this as to other important proceedings in practical surgery.—*Lancet, July* 1 *and* 8, 1871, *pp*, 7, 42.

44.—ON A NEW AND SAFE METHOD OF PERFORMING ACUPRESSURE.

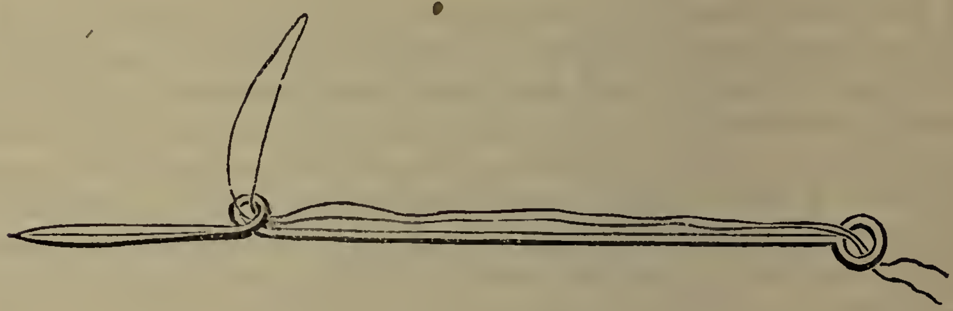
By R. Clement Lucas, Esq., late House Surgeon to Guy's Hospital.

It is now some time since acupressure was introduced at Guy's Hospital; but at the time it was there practised I had an opportunity of observing that it was not unfrequently followed by hemorrhage, either from imperfect application in the first place, or from the wires and pins becoming loose from some cause, or from attempts made to remove them too soon after the operation. I then contrived the method of securing vessels that I am about to describe, which renders acupressure more simple and quite as safe as a ligature, and has the advantage of being equally certain in the hands of a novice as in those of the most expert professor of acupressure. The subsequent introduction of torsion, and the rapid conversion of all Guy's surgeons to that mode of securing vessels, prevented my draw-

ing attention to it at the time; but finding that acupressure is still largely practised and advocated by surgeons of the greatest eminence, I have thought it right to lay this method before the profession.

An ordinary acupressure needle, having a ring at one end, is twisted so as to form a small loop about an inch from its pointed extremity, or at such a distance as the surgeon may think most convenient, varying with the size of the needle. A piece of fine wire is doubled in the middle as in ordinary acupressure, and the loop thus formed is passed first through the ring at the end of the pin, then through the loop on the staff. After sufficient has passed to reach easily over the point of the needle, the loop of wire should be bent up at right angles so as

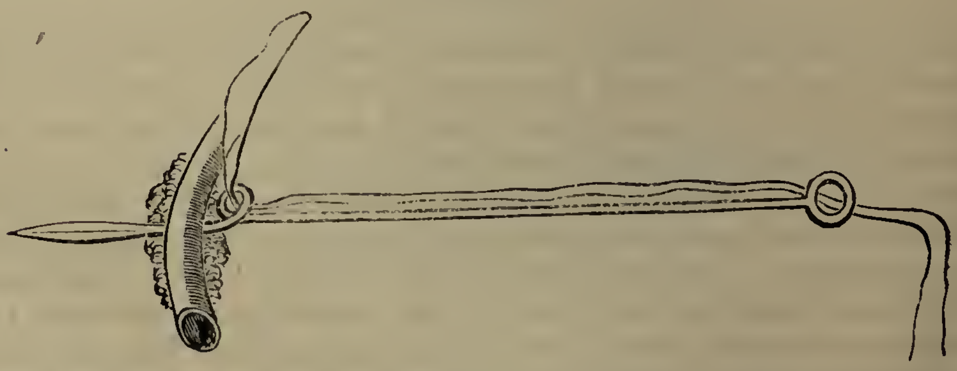
Fig. 1.



to be out of the way, as shown in Fig. 1. In this way needles should be kept ready for use.

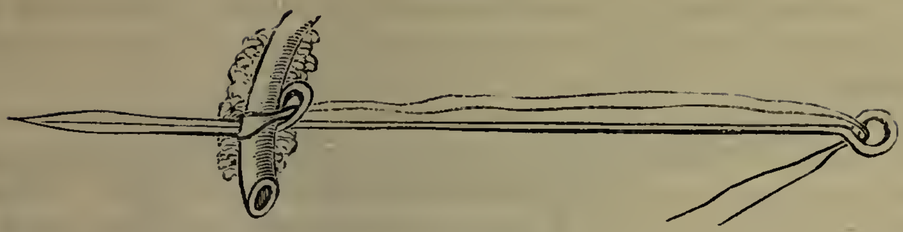
In performing the operation for securing the vessel there are three acts. First, the point of the needle is passed under the vessel, as shown in Fig 2. Secondly, the loop of the wire is bent down over the vessel and slipped over the point of the

Fig. 2.



needle. Thirdly, the vessel is compressed by pulling the free ends of the wire tightly through the ring at the extremity of the needle, and finally secured by bending back the wire at this

FIG. 3.



point as shown in Fig 3; or it may even be made more safe by giving the ends of the wire a twist round the needle close to the ring.

To remove the pin, all that is necessary is to straighten the wire bent round the ring at its outer extremity; then on gently pulling the pin the loop of wire glides along towards the point and releases the vessel. Should bleeding now take place, the vessel may again be compressed by pulling on the wire, provided that the pin has not been so far extracted as to have allowed the loop of wire to pass over its point. After the pin has been removed the wire follows, as in the mode of acupressure described by Professor Pirrie under the name of "circumclusion."

I venture to claim for the above method the following advantages: 1. That it secures the vessel against all chance of bleeding. 2. That it permits the pressure upon the vessel to be regulated. 3. That, when about to remove the wire it allows the surgeon to ease the vessel and again tighten the wire upon it in case of bleeding without reopening the stump, which it is not possible to do by any of the methods now employed."—*Lancet, September* 1871, *p.* 320.

45.—A CONTRIBUTION TO THE SURGERY OF BLEEDING VESSELS.

By Dr. ALEX. OGSTON, Aberdeen.

[About two years ago Dr. Ogston performed a number of experiments in order to test the comparative strength of the closure of vessels by the three methods of ligature, acupressure, and torsion. The result of these experiments was that ligature resisted the highest column of mercury brought to bear upon it, viz., 114 inches, whilst acupressure only resisted 23·5, and torsion 13 inches. This, of course, incontestibly proves the superior safety of the ligature.]

That none of our present modes is entirely satisfactory cannot be denied. Torsion seems, in some hands, to have given mavellously good results; but, having had no opportunity of witnessing it as applied regularly to large vessels, I

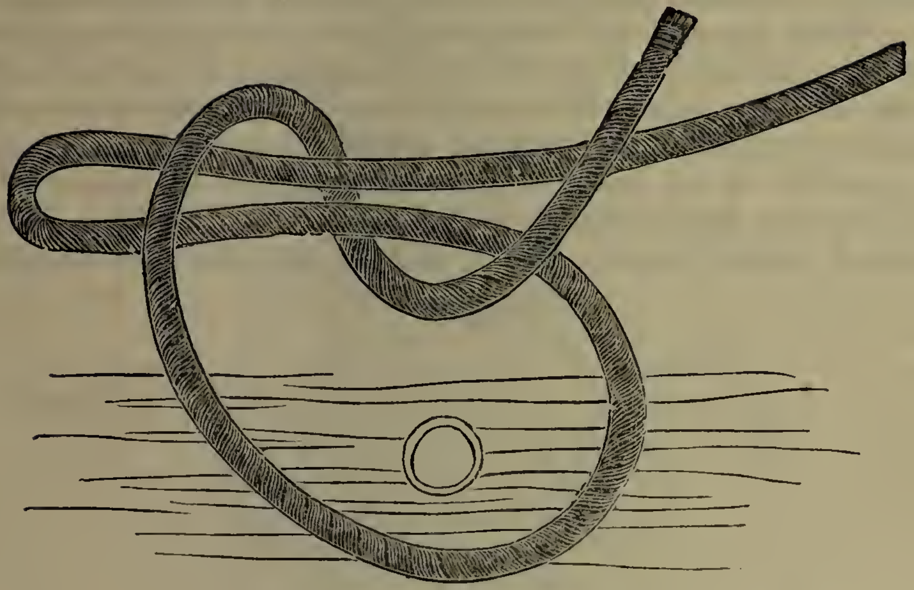
can only say, under liability to correction by those who have tried and adopted it, that the small amount of internal pressure resisted by it forms a serious, although the only, objection to its use. In Aberdeen, on the contrary, we have abundant opportunity of studying acupressure, and the result of my own observation is to convince me that it cannot retain the place originally claimed for it in operative surgery. The one real advantage that it possesses is that it allows the wound to be freed from foreign bodies one or two days after operation —doubtless a very important matter, but which can be attained without its employment. The asserted frequency of primary union in wounds treated here by it, and the absence of suppuration in such wounds, are to a certain extent correct. In amputations of the mamma it yields first class results, primary union and absence of pus being generally achieved; but in other amputations suppuration occurs just as usual, the statements to the contrary being due partly to the great enthusiasm of its advocates, and partly to a careful nurse removing, as far as possible, all traces of discharge before the visit of the surgeon.

Acupressure has several disadvantages. The pins frequently prevent complete closure of the wound at the time of the operation, and the edges have to be accurately adjusted at a subsequent period. The removal of the wires requires a considerable degree of traction, and in the employment of the ring-loop, is sometimes a matter of difficulty, while, in at least one recent instance, an acupressed vessel in an amputation wound required, owing to secondary hemorrhage, the subsequent application of the ligature. The estimate of acupressure I have formed is similar to that of many other surgeons who have given it a trial.

Considering, then, the superior strength of ligatured vessels, it would be a step in advance could the ligatures be applied in such a manner as to retain their superior security, and yet so as to allow of their being removed about the second day after operation, before the tendency to suppuration has commenced in the wound, and also so as to avoid snaring off the ends of the arteries and the tissue grasped by the forceps in the usual mode of applying them.

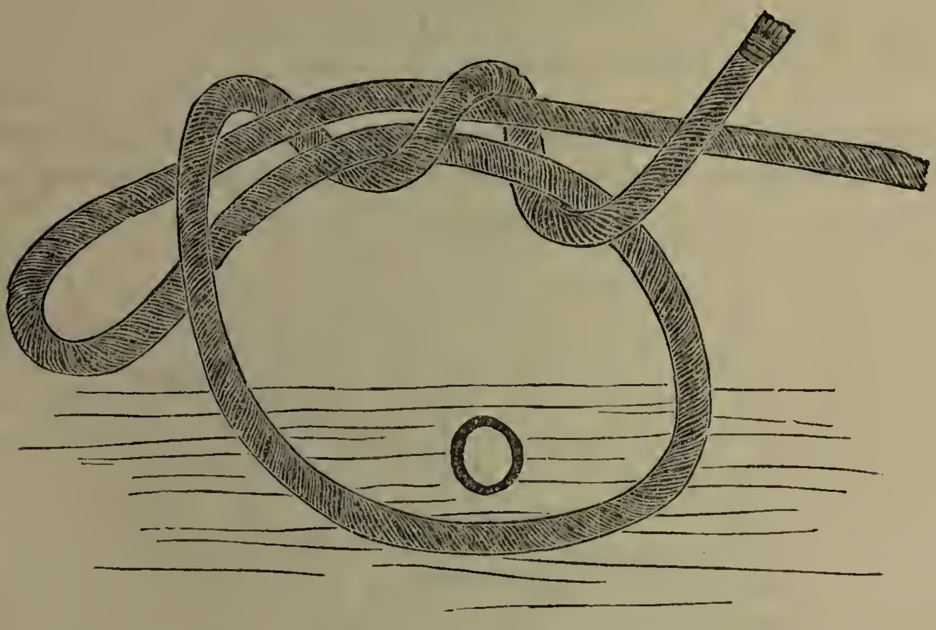
All these advantages are combined in the method I have now to propose—namely, that the vessels be secured by the knot depicted in Fig. 1, which, if it be run close, and the short end be cut off close to the knot, can be removed by very moderate traction on the long end. The knot, it will be perceived, is merely the "thumb or over-hand knot," similar to the first part of the knot ordinarily applied to arteries, with the single exception of a loop of the long end being employed instead of

FIG. 1.



the undoubled cord. Its hold is perfectly sufficient when tied but I shall afterwards come to discuss its security.

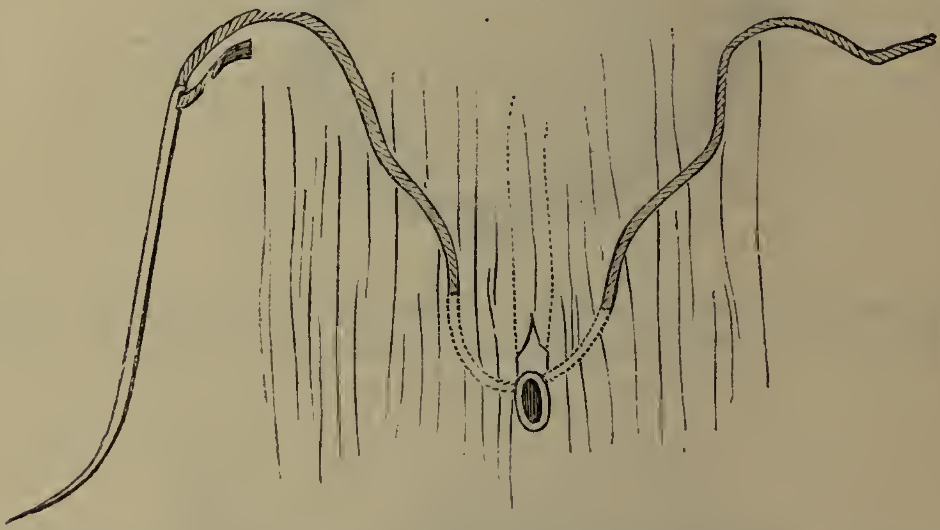
FIG. 2.



In applying the knot to the flaps of an amputation wound, the following is the method I have adopted:—A piece of *the strongest* ligature silk, well waxed to diminish the risk of slipping after it has been tied, has one of its ends passed an inch or thereby through the eye of a curved needle. (A few such armed needles are prepared before the operation.) The bleed-

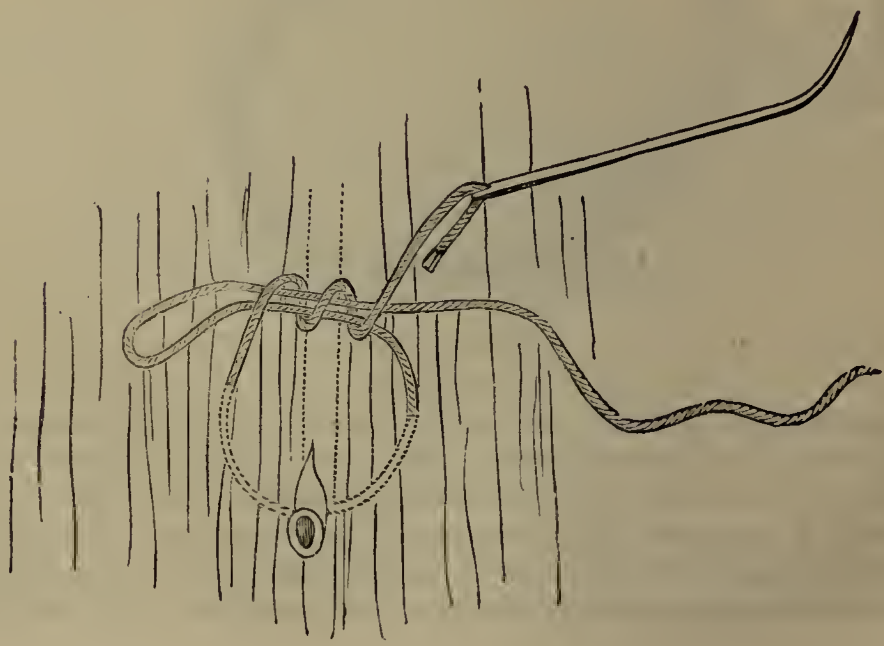
ing vessel being seen, and its course above the orifice estimated, the needle is entered from the raw surface of the flap, a quarter of an inch on one side of the course of the vessel and the same distance above the bleeding orifice; then passed down behind the vessel so as to include it between the needle and the raw surface of the flap; and, having crossed behind the vessel, is brought out a quarter of an inch on the other side of its track. The needle is next drawn through, bringing a couple of inches of the thread with it, as in fig. 3, and it may then be detached

FIG. 3.



fiom the thread. A loop of the long end, an inch or thereby in length, is then used to form, with the single short end, an

FIG. 4.



ordinary "thumb knot," as in Fig. 1, or if additional security is required, a double knot, as in Figs. 2 and 4. The knot is closed upon the vessel and included tissue by traction on the loop and short end; the strands of the loop are placed close to each other, so as to lie in contact throughout and offer no obstacle to removal when the long end is pulled; and, finally, the short end is cut off close to the knot.

Such knots do not slip till traction is made on the long end, which ought to be brought out at the side of the wound next the original point of entry of the needle, and not doubled again over the vessel; and, correspondingly, the loop is laid suitably directed away from the vessel in the direction of the needle when it emerged at the further side of the artery. In small arteries the single knot is amply sufficient; in large arteries I use the double knot for the sake of security. The ligature is removed, on the second day after the operation, by gentle traction on the long end; and the amount of force required for its removal, while too great to be readily effected by accident, is, notwithstanding, less, even with the double knot, than that needed to remove the wire where acupressure has been employed.

The advantages of this proceeding consist in the speedy removal of foreign bodies from the wound, and in the fact that no tissue is compressed or strangled by it save the portion lying in the very knot itself. The distal end of the vessel is still in connexion with the living parts surrounding it, and its nutrition is not interfered with.

The strength of the method is another of its advantages. Vessels experimented upon in the manner given in detail in my former paper, resist, when secured with the single knot, from 24 to 70 inches of mercury; and on an average, 39 inches of mercury, or a pressure of 19 lb. to the squre inch, as against 23·5 inches of mercury with acupressure, and 13 inches with torsion. The double knot resists the full column admitted of by my dynamometer—viz., 114 inches of mercury, or 57 lb. to the square inch. I have tested in animals this application of ligature to cut vessels, and to vessels deligated in continuity, before using it in man, and the results were perfectly satiafactory. In man they have hitherto been equally so. I have at present a stump after Pirogoff's amputation treated in the above way, where the ligatures were removed on the second and third days, and which is healing with as small an amount of discharge as I ever saw obtained, even with acupressure.

The disadvantages are, that, as in acupressure, some tissue is included along with the vessel; and where structures such as important veins and nerves run parallel to the artery, damage might be done by their inclusion. Thus it is best suited for amputations; indeed, without further experience of it, I should

hesitate to employ it in the deligation of an artery in continuity upon a human subject. I have had no opportunity of testing it in that most difficult of all vessels to secure, an interosseous artery in the angle of a flap amputation, but believe that, by laying hold of the bleeding orifice and the tissue surrounding it, and then proceeding, as in a flap, to pass the needle behind it through the grasped mass, it would answer as well as the ordinary ligature.—*Lancet, May* 27, 1871, *p.* 707.

46.—OBSERVATIONS ON LIGATURE OF THE SUBCLAVIAN ARTERY—A NEW INCISION SUGGESTED.

By Assistant-Surgeon F. P. Staples, Medical Staff, Barully, Rohilcund.

That ligature of the subclavian artery on the living subject is not an easy operation, even in the hands of the most experienced surgeons, few will question; while, on the dead body, there is no doubt that most surgeons have seen attempts to occlude that vessel fraught with considerable difficulty, if not with actual unsuccess. To assert such truisms, however, is not my object in writing, but to bring before the notice of surgeons a method which I have practised for some time, and by which, I venture to hope, the difficulties of ligaturing that vessel in the third stage can be overcome.

Operation.—The patient being placed in the usual position, with his head back and to the opposite side, with his shoulder depressed slightly, but not violently, let the point of the knife be entered at the posterior edge of the sterno-mastoid muscle, one inch and a quarter above the superior margin of the clavicle, and let an incision be carried from that point, in a straight line, to within a quarter of an inch of the attachment of the trapezius to that bone, dividing skin and platysma. This incision should be a little short of three inches. The operator should then lay aside his knife, ligature the external jugular vein in two places, and divide it in the direction of the original incision. The deep cervical fascia should now be divided, and the edges of the wound gently separated, when the posterior belly of the omo-hyoid muscle will be exposed for its entire length. The edges of the wound should now be retracted, and the superior retractor should carry with it the omo-hyoideus; and when this has been done, the white cords of the plexus, with the artery inferior and internal to them, will be observed to occupy the bottom of the wound. The knife should now be laid aside, unless it is necessary to dissect a lymphatic gland out of the way, and the vessel separated from the lowest cord of the plexus with a director, and liga-

tured in the usual manner. Tying the external jugular vein is not insisted upon, provided it can be easily drawn aside, but generally a ligature would expedite matters, and any branches of this vein which cross the line of incision should, if divided, be treated in the same manner.

What are the advantages claimed for the operation recommended? Why have the stereotyped guides to the artery—viz., edge of anterior scalenus, and tubercle on first rib—not been mentioned? What special advantages has the operation described over that commonly practised—*i.e.*, by incision along or near upper margin of the clavicle?

The advantages claimed for the operation are—1. That the incision is parallel to the normal course of the artery. 2. That the true guide to the vessel—posterior belly of omo-hyoid—is exposed by incision recommended for its entire length. 3. That the edges of incision admit of easy retraction, and, in this way, of easy access to the vessel. 4. That the risk of venous hemorrhage obscuring the final steps of the operation is lessened.

My answer to the second question I have asked is very simple. To feel the edge of the scalenus anticus in a bleeding wound is next to, if not quite, an impossibility, and it does not follow that the tubercle on the first rib is always so well developed as to permit of recognition by the sense of touch; and, independently of both these surgical signposts, it has always appeared to me that a far more reliable guide is to be found in the omo-hyoideus.

Regarding the third question—What special advantages are claimed for this operation over that commonly practised? It may be stated, I think, that, if an incision is made in a line with the clavicle, it is obvious that, when carried deeper, it will not meet with the omo-hyoideus or true guide unless at its outer angle; whereas the incision recommended is parallel to that muscle throughout its entire length. Secondly, in the wound resulting from the ordinarily used incision, retraction can only be made in an upward direction, as the clavicle prevents retraction downwards; whereas, with the incision now recommended, retraction can be made in both directions. Thirdly, in the incision recommended there is no risk of dividing the transverse cervical vessels; whereas, when the incision along the clavicle is used, they are often cut, and, when it so happens, very troublesome bleeding obscures the further steps of the operation.—*Medical Times and Gazette, July* 22, 1871, *p.* 96.

47.—ON THE TREATMENT OF ANEURISM BY COMPRESSION.

By Dr. Rawdon Macnamara, Surgeon to the Meath Hospital, &c.

A case of popliteal aneurism presents itself for treatment. We determine to use compression. We first carefully ascertain the condition of the patient's general health. If anæmic or hyperæmic, we take appropriate measures; and, when we are satisfied upon this point, we apply some one or other of the most improved compressors—those in which the compressing power is modified by elasticity. With this we compress the artery in the upper portion of its course, having previously arranged, some three or four inches lower down, the auxiliary instrument by means of which we propose to alternate the pressure. The upper instrument is now made to control the artery, so as but just to arrest the pulsation in the sac. This is the most delicate step in all the procedure, and is regulated by the hand of an intelligent assistant, who at once informs us when the pulsation is arrested; and then and there the further application of pressure is arrested. A roster of intelligent students is now organised, and to them is entrusted the management of the case. Two are appointed to take charge of the patient for one hour, when they are relieved by two others, and so on during the day, whereby we secure unwearied attention during the period that pressure is kept up; and, as in Dublin we visit our hospitals at 9 o'clock a.m., the treatment generally commences about that hour, and is continued up to 9 o'clock p.m., when all pressure is removed, and the patient is encouraged to take his night's rest undisturbed. Next morning the treatment is resumed, and so on until the cure is perfected. At the commencement of the case, we take the patient into our confidence; explain to him the nature of his case and the method we are about to adopt for his cure, placing clearly before him the alternative, with all its possible dangers, which we should have to adopt in case compression should fail. The value of this procedure is very frequently demonstrated by the intelligent interest exhibited by our patients in the management of their own cases—so intelligent as in protracted cases to supplement, if not altogether to supersede, the supervision of them by our students. In the selection of our compressing force, we adopt in its widest sense the maxim "Nullius addictus jurare in verba magistri." Should one compressor prove irksome, we try another; if all should fail, we have recourse to digital compression, or to compression by means of weights; but, in every instance convinced of the soundness of this plan of treatment, we leave no stone unturned to secure its success.

As you will collect from the preceding remarks every exertion is made on our parts to conduct the case with as little inconvenience or pain to the patient as is possible. Pain in the seat of pressure we look upon as an evil; but there is a pain, and that sometimes for a few hours a very sharp pain, to the advent of which we anxiously look forward. I allude to pain in the neighbourhood of the knee-joint, which pain is the harbinger of cure. This pain is due to the rapid enlargement of the collateral circulation, dependent upon the diminished calibre of the aneurismal sac, encroached upon by the deposit in it of fibrine, and is best combated by narcotics.

So far for our Irish plan of treating aneurism by compression: I now come to the question so often asked me, why we should be so successful, and you so much the reverse, in thus treating aneurism.

First. And above all, we have faith in the efficacy of our treatment, and, reverently be it expressed, faith can do all things. Your experience can scarcely justify you in entertaining such faith.

Second. In this instance, at all events, the spirit of nationality pervades our ranks, and the humblest, the most indolent, the most apathetic surgeon amongst us, will try every expedient ere he will acknowledge that in his hands the Irish method of cure has failed.

Third. Our hospitals are visited at 9 a.m.; yours, I believe, at 2 p.m. By our arrangements we are enabled to visit our patient, and to commence his treatment under our personal supervision at an early period of the day.

Fourth. All our hospitals are within a few minutes' drive of the residences of their surgeons; thus are we enabled to pay during the day repeated visits to such cases, and so to watch them from hour to hour; and no matter how zealous, how intelligent, be the house-surgeons or the pupils, there is no eye equal to the master's for seeing that the work be done. That such surveillance imposes great trouble on the attending surgeon I freely admit, still it must always be borne in mind that it was *per tœdia et labores* that this plan of treatment has been brought to its present perfection, and that *pour reussir il faut travailler*. You know best your arrangements in these respects.

Fifth. Thoroughly aware, as we are, of the pathological changes which must be brought about in the contents of the sac to effect a cure, and convinced, as we are, of the principles whereby alone these changes can be produced, we are not afraid, during the night, to remove all pressure, and so to secure for our patients uninterrupted sleep, whereby we keep

up their health to the condition most favourable for fibrinous deposit in the sac.

Sixth. And here I trust that I will not be considered as blowing too loud a blast on our Irish trumpet, I am of opinion that some of our success must be attributed to the intelligence of the patients whom we are called on to treat. Many are the instances in which their co-operation has proved to us of inestimable value. One of the class of instruments (the clamp) of which, even to the present day, we frequently avail ourselves, is due to the suggestion of a patient (a carpenter by trade) of the late Professor Harrison's; and, did time permit, numerous other instances of the truth of this assertion could be adduced.

Finally. The physique of our patients differs widely, I suspect, from yours. I fancy that the disease occurs, in the majority of English patients, in the persons of well-fed, robust, and plethoric individuals; such patients will require a compressing force of high tension; but, as a rule, it may be laid down that if the force employed exceed that which would be represented by a *dead* pressure of nine pounds, the pain to which it will give rise will soon render it intolerable. Our patients rarely, if ever, present such appearances; they rather incline to the opposite extreme—and, when such is the case, we are obliged to call to our assistance good diet. 'Tis true that the plethoric individual may be reduced by regimen, &c., to the *juste milieu.* Still, after all, it is but human nature that we should find patients more amenable to a plan of treatment which involves generous diet, than to one which enforces starvation and depletion,—*Brit. Med. Jour., Aug.* 19, 1871, *p.* 200.

48.—ON ATHEROMA OF ARTERIES.

By Dr. J. Milner Fothergill, late Senior Resident Medical Officer to the Public Dispensary, Leeds.

Atheroma is essentially a parenchymatous inflammation of the inner arterial tissues, and may either exist in patches or be more general, leading to dilatation of the arteries, and loss of elasticity. When the arterial elasticity is impaired, the artery distended by the ventricular systole is unable to recover itself. It thus remains more and more distended, and, when possible, elongated—*i.e.*, tortuous. This impaired elasticity then lessens the recoil; the distension and elongation of the arteries interfere with the recoil; and, instead of perfect recoil, the artery remains permanently enlarged. This change goes on gradually, incessantly aggravated by exertion, and all calls on the heart for increased action. The arteries become elongated and dis-

tended, from the aorta to the tortuous temporal artery; they are increased in length and breadth; they become, too, more brittle and more easily ruptured by strain. Here, now, we see a wonderful instance of conservative change, tending directly to prolong the existence of the organism. The diseased arteries recoil less perfectly, and thus the coronary circulation becomes involved, and its circulation diminished, and thus impaired nutrition of the heart; and from that, again, lessened and impaired activity, and thus diminished risk of the heart rupturing the diseased and friable arteries. Thus we see that not only is fatty degeneration a preservative lesion, as Sir William Jenner showed at Leeds, but that that very change which is preservative in its action, is entailed by the diseased condition of the arteries, which the very want of that lesion would endanger. Thus the changes go on, hand in hand; the more diseased, brittle, and non-elastic the arteries, the less their power of recoil, and thus impaired structural integrity of the heart and impaired contractile power. The diseased arteries thus fortunately lead to the very muscular change imperatively necessary in order to prevent the heart from rupturing them. Where the atheroma is general, there is a stronger probability of fatty degeneration following, and thus this condition is more favourable to life than when the atheroma is in patches. Where the atheroma is deposited in ring-like, annular patches, encircling the aorta at the branching off of some smaller artery—its favourite seat—it is more fraught with danger. The hypertrophy consequent on the obstruction to the flow, and increased tension resulting from the obstruction, does not so soon undergo preservative degeneration, as the sound parts preserve their elasticity and recoil fairly. Thus the obstruction of the isolated masses, especially if annular, keeps up the hypertrophy by increasing the tension behind them and the results of increased arterial tension, and thus endangers the patient's existence by the increased risk of some degenerate mass giving way. In these conditions, increased ventricular action could only render them more and more dangerous; the greater contraction and distension would try still farther the degenerate vessels, and could only slightly improve the coronary circulation, and thus only to a small extent diminish the risk of arrest of the heart's action, while it would certainly greatly enhance the more imminent risk of arterial rupture. If ever hypertrophy is to be regarded as otherwise than an unalloyed good, it is in these conditions. Certainly any attempt to secure it artificially in the manner described, would be detrimental to the patient's real interests; as the attempt to extenuate one danger would greatly enhance the hazard of another.—*British Medical Journal, July* 15, 1871, *p.* 58.

49.—NEW OPERATION FOR THE CURE OF VARICOSE VEINS.

Mr. Stokes has been recently treating varicose veins on a plan which was suggested to him by Sir Dominic Corrigan. It occurred to Sir Dominic that, as hemorrhoidal tumours are, as a rule, so successfully treated by the application of strong nitric acid, the application of this acid to varicose veins in other situations would probably be attended with equally good results. In a case which is still under observation in the Richmond Hospital, this plan of treatment has been attended with the happiest results. The patient is a young man aged 21, and was admitted into hospital on the 15th of last month. He had a varicose tumour, of the size of a small orange, on the inner aspect of the middle third of the right leg. It had existed for seven years. He suffered also from a large varicose ulcer, which existed over the inner ankle of the same leg; and there was also a second tumour, formed of a cluster of varicose veins, in the right groin. Mr. Stokes performed the operation in the following way. Pressure having been made above and below the tumour, the integuments were raised from the tumour, and an incision, by transfixion, was made over the veins. The *fuming* nitric acid was then applied to the external coats of the veins. No pain attended this application. On the following day, the contents of the tumour appeared solidified at the base; and the acid was again applied. The process of solidification then went on rapidly, the tumour at the same time decreasing in size. A week after the operation, some coagulated blood appeared at the site of the operation; and the following day a portion of the vein came away. This was followed by a slight local inflammation; which, however, after a few days, quite subsided. The wound was then for some days dressed with tinct. benzoin co. and glycerine; and it rapidly healed, as did also the ulcer; and the large varicose tumour in the groin entirely disappeared.—*British Medical Journal, May* 28, 1871, *p.* 550.

ORGANS OF RESPIRATION.

50.—ON PARACENTESIS THORACIS.

By Dr. H. W. Fuller, Physician to St. George's Hospital.

[So lately as when Dr. Fuller held the office of Medical Registrar to St. George's Hospital, paracentesis thoracis was rarely practised—never except when the probed needle showed that

the effusion was purulent. Even then the operation was not unfrequently delayed too long, until emaciation and hectic proved that the patient must soon succumb to the malady.]

Nowadays the folly of unduly deferring the operation is clearly recognised, and the tendency is to operate prematurely, before the improbability of producing absorption of the fluid has been ascertained, rather than to subject the patient to unnecessary delay. But there are certain points relative to the operation about which even now erroneous notions are entertained. The fallacious dread as to the admission of air into the pleural cavity which formerly exercised such a pernicious sway as almost to preclude the operation of paracentesis, even now often leads to the failure of the operation. Under the impression that the momentary admission of air will set up fresh inflammatory action in the pleura, or will injuriously modify existing inflammation, mechanical contrivances of various kinds have been employed to draw off the fluid from the chest without admitting air. The chest has been tapped under water; elastic tubes have been attached to the canula, and made to terminate under water; air-tight elastic bags have been attached to the canula—and various syringes have been made use of connected with an elaborate system of stopcocks. But whatever their form or precise character, they have one object in common, namely, to prevent the admission of air; and their advocates have asserted, not only that they effect their object, but that the chest may be readily emptied of its fluid contents by their agency. My objections to their use are both practical and theoretical. I object to their employment, 1st, because they are unnecessary, and complicate a very simple and harmless operation; 2nd, because the admission of air during the process of tapping causes no injury to the patient; 3rd, because it is impossible by any contrivance to prevent the admission of a certain quantity of air during the withdrawal of the canula—and, therefore, even on theoretical grounds, there can be no valid reason against the admission of a larger quantity; 4th, because they mostly occasion unnecessary pain, and when suction-syringes are employed, a forcible strain is put on the parts, which is not only felt and complained of by the patient, but in some instances sets up fresh and serious local inflammation; 5th, because, although a certain quantity of fluid may be drawn off, it is physically impossible to *empty* the chest by their agency; and observation at the bedside has convinced me, that recovery takes place less frequently when a small quantity only of the fluid is drawn off than when the chest is thoroughly emptied. On one of these points only will I make any farther remark, viz., the impossibility in most instances of emptying the chest without

the admission of air. When effusion into the chest has taken place rapidly, and tapping is had recourse to early, while the lung is still capable of expanding freely, it might be possible to evacuate the fluid contents of the chest without admitting air, if only some mechanical means could be devised for the purpose. In the case here suggested the lung would expand to fill up the space previously occupied by the fluid. But in many instances which occur in practice the pleurisy is of old standing; the lung has been compressed by fluid for weeks, or even months; it is carnified and incapable of speedy expansion; in many instances it is so bound down by firm adhesions, that expansion can only be brought about by the natural inspiratory efforts, after a period of many months. In these cases, which are very numerous, it is simply impossible, whether by a suction-syringe, or by any other means, to draw off more than a small proportion of the fluid without the admission of air. The chest is a closed cavity like a cask; and just as it is impossible to empty a cask by tapping it without admitting air, so also is it in respect to the chest. The only difference between the two cases is occasioned by the rigidity of the walls of the cavity in the one case, and by their pliability and yielding character in the other. The walls of the cask being rigid, its fluid contents will run out only to the extent to which air can find admission to supply their place; whereas the walls of the pleural cavity will yield in some measure, to supply the place of fluid which is drawn off. The chest-walls will fall in, the diaphragm will rise, the mediastinum will encroach on the affected side; and up to the point to which they are capable of yielding, there is no necessity for the admission of air, inasmuch as by their displacement the space previously occupied by the fluid which is drawn off is at once filled up. But, provided the lung is permanently or even temporarily incapable of expanding, it is physically certain that, without the admission of air, the fluid contents of the pleural cavity can only be drawn off to the extent to which the walls of the cavity are capable of falling in to supply the place of the fluid withdrawn. If, in contravention of this physical law, an attempt is made by forcible suction-syringes to draw off still more of the fluid, injury to the patient must ensue; for there must be a forcible dragging and stretching of the walls of the chest, or of the lung itself, corresponding to the extra amount of fluid withdrawn. And as all mechanical violence is necessarily hurtful, it is obvious that in using forcible suction a considerable risk of injuring the patient is incurred, without the slightest corresponding advantage.

Thus, then, I come to the conclusion, on theoretical grounds, that all mechanical contrivances devised to exclude the air, or

to exercise forcible suction of the fluid contents of the chest, are useless, or worse than useless; and this conclusion has been confirmed by my bedside experience; for I have seen patients injured in this manner. On the other hand, I maintain that the temporary admission of air is of little or no importance; and that if only a free opening is made into the lower part of the chest, whether by a full-sized trochar or a scalpel, the operation of tapping is a simple one, and almost uniformly successful.

From the time of my appointment as physician to St. George's Hospital, I have tapped every case of pleurisy, which has come under my care, in which, either from the urgency of the symptoms or the difficulty experienced in producing absorption of the fluid, recourse to more active measures seemed desirable; and out of the large number of cases in which tapping has been performed, one only has proved fatal. Within the last six months you have seen four of my cases tapped; no attempt was made to exclude the air during the operation, and yet you know how rapidly and satisfactorily they all recovered.

My advice, then, founded on large bedside experience, may be summarised thus: 1st, tap whenever dyspnœa is very urgent, or as soon as it becomes evident that remedies fail to produce absorption of the fluid in the chest; 2nd, tap as low down as possible, and make a free opening, allowing the chest to empty itself thoroughly; 3rd, so far as possible, avoid causing any local irritation; 4th, if the fluid withdrawn is serous or sero-sanguineous, close the opening with carbolic plaster as soon as the operation is concluded; if, on the contrary, the fluid is purulent, adopt some means to prevent the wound from closing, and take care that the matter is allowed to drain off as fast as it is formed; 5th, after the operation, support the patient by bark and good nourishment, and for a day or two give him opium if necessary.—*St. George's Hospital Reports*, 1870, *Vol.* v., *p.* 7.

ORGANS OF URINE AND GENERATION.

51.—ON THE CHOICE OF OPERATIONS AT ALL AGES FOR STONE IN THE BLADDER.

By SIR HENRY THOMPSON, Surgeon Extraordinary to His Majesty the King of the Belgians, Surgeon and Professor of Clinical Surgery to University College Hospital.

[The following paper is founded upon twelve cases of stone recently in the wards of University College Hospital. Four

were treated by lithotomy and eight by lithotrity. Why were the four selected for one mode of treatment, and the eight for the other?]

My aim, then, is chiefly to answer for you the following question: "How do you select for each operation, respectively, the cases best adapted to each method?"

I reply, it is essential, in order to form a judgment, that you must previously become acquainted with the following particulars:—1. The size of the stone; or the amount of calculous matter, if multiple. 2. Its composition. 3. The age and constitution of the patient. 4. The conditions of the local organs.

First, the size of the stone. This is the most important feature, taken alone; and the earliest examination of a case should inform us accurately on this head. For if a stone is small, it is almost certain to be successfully disposed of by lithotrity; and if it is very large, it will certainly be incurring a considerable risk to depend on the lithotrite for its removal. Were the alternatives always so complete as this, there would be no difficulty in determining the question at issue; but, as you well know, there are stones of all sizes between these two extremes, and many exist on the border-ground, so to speak, of size, for which other circumstances than that of magnitude may influence a decision in the case. To speak approximatively, however, in the matter of size, I think I may fairly say that a stone which measures $1\frac{1}{4}$in. to $1\frac{3}{8}$in. in its longest diameter is for the most part fairly amenable to lithotrity. A stone of which any measurement is $1\frac{3}{4}$in. is mostly too large.

But how, it may be said, do you determine these measurements? You who accompany me round the wards know that, at my first or second visit to any case of stone, this quality of size is easily ascertained by means of a flat-bladed lithotrite. I catch the stone first in one diameter and then in another, and the distance between the two blades is easily read off at the handle. There are other and subordinate ways—such as with the sound itself;—but none are so certain or so accurate as this, and it should always be employed. At the same time, you determine whether other stones exist besides that which lies within the grasp of your blades, by feeling for them while still grasping the stone already caught.

But you want, secondly, to know the nature and composition of the stone. This is really scarcely less essential to know than its size. Remembering, then, that three-fifths of all stones, speaking roughly, are composed of uric acid and the urates, the other two-fifths phosphatic or mixed, and only now and then an oxalate of lime is met with, you will be prepared to expect a uric-acid stone if no circumstance contraindicates

its presence. But, further, if in an ordinarily healthy man of from fifty to seventy years you find a hard stone, it is 7 to 1 that the stone is composed of some form of uric acid, pure or with a base; but if he is a strong, hale, hearty man of that age, and his urine is clear, it is 15 to 1 that the calculus belongs to this class. An oxalate-of-lime stone is more likely to be found at an earlier period of life, and the man has probably a less healthy appearance. If the stone be phosphatic, the patient is often broken in health, and the urine mixed with mucus, pus, phosphates, and a little blood. Next, there is the evidence to be derived from the "feel" of the stone by the sound, and the note which it utters when you give it a short, sharp tap. With uric acid and oxalate of lime the note is clear, and on drawing the beak of the sound along it a firm, hard surface is felt. But this is by no means decisive: you must feel your stone in the jaws of the lithotrite, and this will tell you more. The "feel" of an oxalate-of-lime calculus is wholly different from that of uric acid. I grant that this is appreciable rather to the practised hand than to that of the tyro, still I can describe perfectly what I mean. Suppose you have a good-sized uric-acid stone in the blades of your lithotrite; on turning the screw a little, and gently, you can easily feel the blades "bite"—that is, sink a little into the crust of the hard body; but if you have an oxalate-of-lime calculus in the blades, on screwing you make no impression the stem of the male blade recoils with a spring, there is no "bite;" it is as though you had a piece of unyielding metal between the blades, not like a bit of some friable rock, which the other more resembles. On the other hand, a phosphatic stone gives a soft, uncertain sound when struck with an instrument, and the sensation of a rough, gritty body to it when drawing along its surface the beak of the sound; and the "feel" in the blades is also soft, gritty, and yielding. Indeed, with a little attention to these matters, and to the state of the urine, there is little or no difficulty in ascertaining the nature of a stone. It is quite true that in a few exceptional cases you may have a uric-acid stone or an oxalate-of-lime stone coated thickly with phosphates, and that you may be deceived thus; but these are rare instances, and by no means necessarily interfere much practically with the object we have in view—viz., the determination of the nature of the stone before operating. Thus, a sitting or two by lithotrity will soon remove the phosphatic crust, and a hard, unyielding mass of oxalate of lime may be arrived at, which must be extracted by lithotomy unless it be small.

Thus far we have considered under two heads the character of the stone itself. We will now study the questions as affected by the condition of the patient in regard of age and constitu-

tion. First, then, as a rule, to which there are very few exceptions, no proceeding is so perfect for all cases below puberty as lithotomy. In the series before us, however, one of these exceptions has fortunately occurred, which I shall refer to hereafter. The best information we have enables us to say that the deaths are not more from lithotomy than one in fifteen cases below the period of puberty, and in infancy they are still fewer. All these are consequently so far disposed of. For adults only, therefore, the question of choice remains. I may say, then, that *for a small stone, whatever the age*, without any hesitation, lithotrity is the operation. I have to repeat that I have never yet lost a case of small stone by lithotrity. I mean by "small" the size of a bean or small nut—removable, say, at two easy sittings. This is a fact of the utmost importance, and the practical value of which it is impossible to overrate. Every stone ought to be detected by the time it arrives at the size indicated; and, this being so, it is possible to remove it with a certainty of success and without risk to the patient's life.

Thus far no difficulty exists in our choice. But it is when we come to stones of an inch and upwards in diameter that other circumstances demand consideration. It is here that we look for other lights to guide us in our decision. Now I must remind you that in former time it was usual to admit that several conditions existed which were very unfavourable to lithotrity, and which, it was held, should incline the operator to prefer lithotomy. Among these, the strong tendency to rigors after almost any instrumental interference, supposed to depend on some derangement of the kidneys, was regarded as strongly contra-indicating the crushing operation. Now I think we may say it is very doubtful whether the phenomena so referred to are to be regarded as putting aside lithotrity altogether. I have of late given much consideration to this subject of "urethral fever," as it has been termed, although I have not time to say much about it now. I have long observed, in common with others, the strong tendency which exists in some persons to manifest constitutional disturbance after even very slight mechanical interference with the bladder and urethra. It has been the habit to say, and I have often said it, that this tendency, when strongly marked, indicates perhaps the existence of some degree of mischief in the kidneys. I confess I doubt this. I have yet to learn that this tendency is necessarily or even often associated with renal disease. It is true that if disease of those organs exist, dangerous fever is apt to arise from an operation of lithotrity, or from any slight mechanical interference. But that a very strong tendency to the occurrence of what has been termed "urethral

fever" does exist in many who have no sign of renal disease is to me equally certain. Understand that I mean by this "fever" not ordinary surgical fever, nor pyæmia, and of course not any specific fever, as typhoid, &c., but an occurrence of the following phenomena: Within a varying period of time after the use of an instrument, it may be after the first micturition, or after twenty-four or forty-eight or even seventy-two hours, the interval being often one of apparently perfect health, the patient is attacked with rigors, followed by dry heat, then sweating, often with much pain in the back, limbs, and head, and considerable depression of the vital powers, the urine being scanty and concentrated. This paroxysm has sometimes a tendency to recur, and is uninfluenced in this respect, as far as I can judge, by quinine. The occurrence of these events appears to me to be more an affair of constitution or temperament than of the presence or absence of renal disease. These symptoms appear readily among certain excitable or sensitive people, and I think I am right in saying they are more common among patients who are of Celtic than of Anglo-Saxon origin, using these terms in the broad general sense in which they have of late been usually understood. I say this advisedly, after a good deal of experience of patients among the two races, and I am quite satisfied that when I have to do with one of marked Celtic type I am likely to meet with the phenomena described after the most delicate mechanical interference. Such bear pain badly too, dread an instrument excessively, are impatient of control, and lose vital power rapidly under the influence of continued slight irritations. People with this constitution, whatever their nationality, do not well bear numerous sittings, nor the trouble of passing fragments, are apt to lose pluck and hope, and will be exhausted by pain and loss of rest before the numerous sittings essential for a rather large stone can be accomplished. I think, then, that this excitable temperament, this disposition to the fever I have spoken of, are the most unpromising conditions we can encounter in a patient with such a stone, so far as lithotrity is considered. And I cannot promise you that lithotomy is much better suited to them; more observation is necessary; but I am inclined to believe that when you are so unfortunate as to meet with a large stone in one of these people, you had better cut than crush. It is to be remarked, however, that it is always some one's fault if such a patient is met with having a large stone. He is sensitive, soon complains, and his stone ought to be found when it is small. There is little excuse for missing it. These patients are easily identified, not merely by the general evidences of excitability which they

readily manifest, but by the tendency to rigor after the mere preliminary sounding.

Next, we have to consider the condition of the urinary organs in aiding us to determine the choice of operations. In both operations a healthy condition, or nearly so, of these organs is of course more favourable than the contrary. What are the deviations from health most commonly met with in adult calculous patients? 1. Chronic cystitis. 2. Enlargement of the prostate. 3. Inability of the bladder to empty itself at the act of micturition. 4. Stricture of the urethra. 5. Pyelitis and renal disease.

1. Chronic cystitis is by no means always present; but, when it is, the removal of the stone mostly, not always, suffices to effect its cure. When this result does not follow the operation, the bladder probably does not empty itself. (To be considered under No. 3). Only when very severe and of long standing is chronic cystitis to be considered in connexion with our special object of inquiry.

2. Enlargement of the prostate rarely opposes any obstacle to lithotrity. In some of the most favourable cases I have had, and with the largest stones to which I have ventured to apply lithotrity, this condition has been present in a marked degree. The form of the lithotrite is particularly well adapted to enter the bladder easily when there is much enlargement of the prostate. There is not necessarily engorgement of the bladder and retention of urine, although often it is associated with—

3. Inability of the bladder to empty itself by the natural efforts. With this there is often associated more or less of chronic cystitis. If a patient passes a part of his urine, still more if he passes the whole of it, by catheter, troubles of a grave kind *may* arise in lithotrity, although this by no means necessarily happens. Those who are accustomed to daily catheterism bear instruments, and consequently lithotrity, remarkably well; and not only that, but they bear the withdrawal of the débris—a proceeding essentially necessary in these cases. since it cannot be passed by the patient's efforts. If the stone, then, is not large, and the cystitis is but slight, no difficulty need be apprehended. But if the bladder is irritable, the patient requiring to use his catheter frequently, lithotrity, and the presence of fragments, will increase the irritation; and he will require the catheter at such short intervals that his rest is greatly broken, the urethra becomes very tender, and he runs the risk of making false passages, or at any rate injuring the canal, perhaps loses power to pass his instrument properly, and his position is then one of extreme danger. A patient, therefore, who requires very frequent catheterism, whose urine is loaded with thick organic deposit, and whose stone is above an

inch in diameter, as a rule, to which there are exceptions, had perhaps better be cut. If the stone can be removed at two, or at most three, sittings, no doubt lithotrity would be the safer operation. If six or seven should be requisite, perhaps lithotomy will afford him a better chance.

4. Stricture of the urethra is by no means an obstacle unless the stone is rather large. The method of meeting the difficulty will be mentioned in remarking on one of the cases.

5. Renal disease and pyelitis are unfavourable conditions for either operation. When the stone is small, lithotrity ought to be held responsible, and I accept the onus of removing such by crushing, although great care is necessary. The intervals between the sittings must be long, and the patient must be very carefully tended and watched.

Now for a few words touching the mode of lithotomy employed. All those in our list have been operated on by the lateral method. A few years ago you know the median operation for a time came into favour, or, more correctly speaking perhaps, an extended trial was accorded to it by many surgeons in this country, myself among them. Confessedly, however, even on the part of its advocates, it was not considered adequate to the removal of large stones; and as meantime lithotrity has been advancing, and has become more and more extensively applicable, the want of an operation for small and middle-sized stones has disappeared. The only cutting method which, as a rule, is necessary now is one by which we can most safely and easily extract stones of rather large size, almost all beneath these being amenable to lithotrity. For this purpose lateral lithotomy is, doubtless, preferable, and, excepting only for some unusually large calculus, to which a high operation may be better suited, is almost invariably applicable both to adults and children. Hence I have applied it to each of these four cases of two years, sixteen years, and two of sixty-five years with unexceptionable results in every case. The only drawback was in one in which, owing to various circumstances, a slight degree of sloughing took place between the wound and the rectum in the second week after the operation. This was a case in which there was much deformity from anchylosed hip; in it there was much hemorrhage, and a tube had to be introduced, with tight plugging round, to arrest the bleeding; and the stone in this case weighed nearly three ounces. When the patient left the hospital but a few drops of urine passed by the rectum, and the quantity was steadily diminishing. In the other cases no tube was employed; indeed now, unless bleeding takes place, in which case I use a "petticoated" one, well stuffed with lint, I have discarded this instrument altogether. I think its presence hinders the

healing process, and often causes unnecessary and additional irritation of the bladder.—*Lancet, July* 22, 1871, *p.* 115.

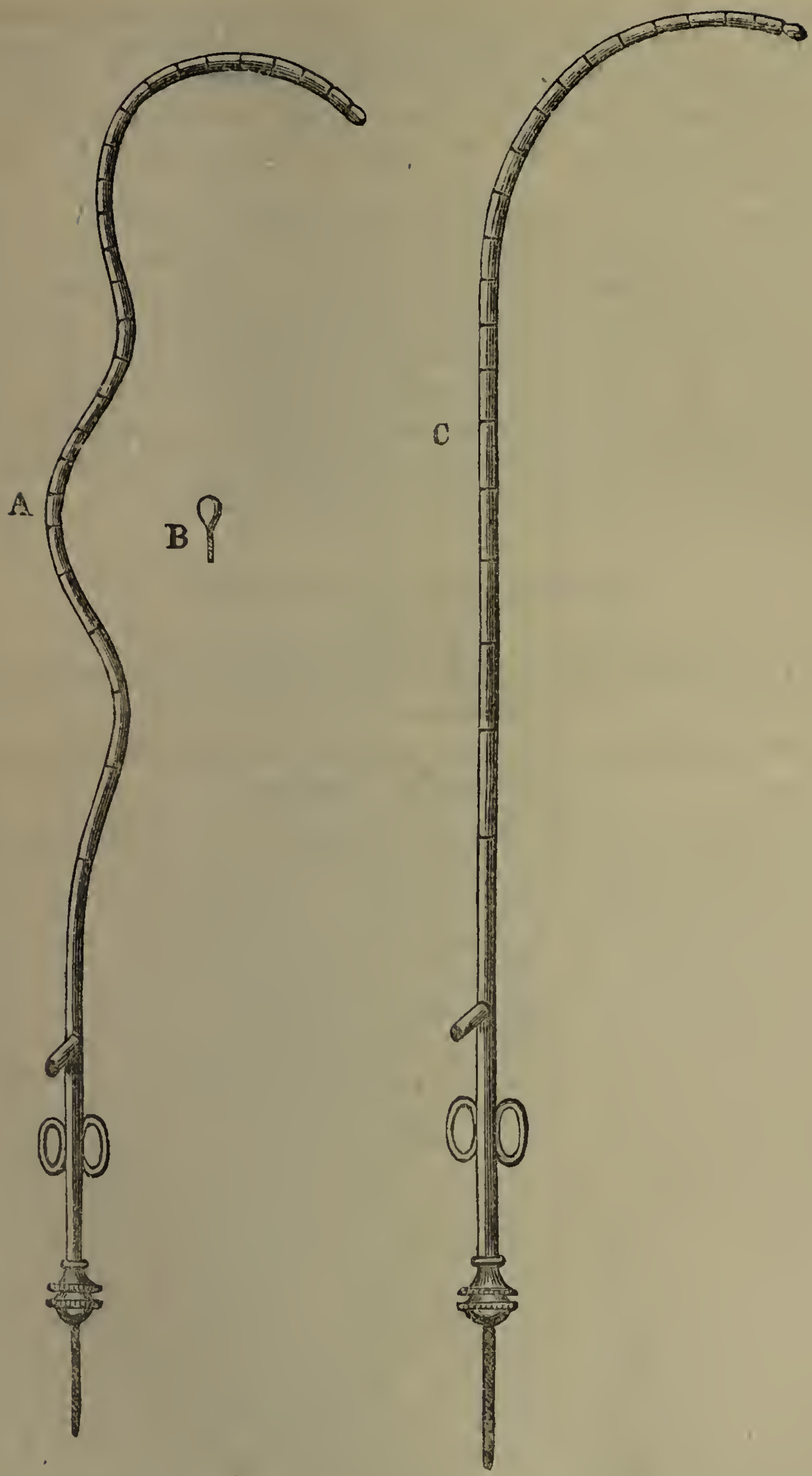
52.—SAYRE'S VERTEBRATED PROBE AND CATHETER.

By Dr. Lewis A. Sayre, Surgeon to the Belle Vue Hospital, New York.

[The following is a letter to the *British Medical Journal*, in consequence of requests from different surgeons and instrument makers to examine Dr. Sayre's Vertebrated Probe and Catheter for detecting dead bone in tortuous sinuses, and for drawing off urine in cases of enlarged prostate, and also for use as a bent probe.]

It consists simply of a series of hollow silver disks, made a trifle smaller at one end than at the other, so as to fit into one another, like a pile of cups or tumblers. These are held together by a linked chain running through the series, and jointed nearly opposite each disk-insertion. This chain terminates in a square rod, which runs through the last disk, and is much larger than any of the others; and on the end of the small rod is cut a thread on which runs a small button-screw, which can make the chain tight or loose at pleasure. Of course, when the screw is turned back, the chain being lengthened, the disks fall away from each other, and the probe is as limber as a chain, capable of following any sinuosity into which it may be pushed. And by a few turns of the screw, the chain being shortened, the disks are drawn firmly together, so as to make a *solid* probe, which will give the concussion against carious or necrosed bone, the same as any other probe. A small slot is made in the canula containing the screw, for the purpose of putting a small nut which regulates the tension of the chain, and thus prevents any possibility of applying sufficient force to break it. There are two fenestræ in the distal disk, for the purpose of drawing an oakum seton through deep sinuses and carious joints: this makes it also very useful as a catheter in cases of tortuous urethras from enlarged prostate. It is impossible to make a false passage with it; and, as it is simply a series of ball-and-socket, or universal joints, it will follow any passage, however devious. By simply unscrewing the steel bulb at the end, and inserting a bulb of porcelain, according to the suggestion of Nélaton, you have the most perfect bullet-probe that can be desired.

I have used this probe for more than seven months, and found it of the greatest advantage in the three several cases for which I have recommended it. I presented it to the New York Medical Society in July last; and an account of it will be published in



their Annual Reports this year. But until now it has not been published to the profession, except in my public lectures at Bellevue Hospital, where I have used it frequently with great

advantage. Mr. Weiss of the Strand is now making a number of them, by the order of Sir Alexander Armstrong, Director of the Medical Department of the Royal Navy; and I am happy to say that he has copied the model very admirably. Of course, like all other probes, this one requires to be made of different lengths and sizes.

To clean it, it is necessary to unscrew it at the end, and to remove the small screw at the slot in the canula, when it will immediately fall to pieces. After washing, it is easily put together, just the same as a string of beads, only remembering to put the small end of the disk on the wire first; and, as each disk increases in length until the end, of course no error can occur in making them fit properly. The cut, together with this description, I think, will make it plain.—*British Medical Journal, July* 22, 1871, *p.* 109.

AFFECTIONS OF THE EYE.

53.—CATARACT AND ITS TREATMENT BY SEMILUNAR CORNEAL INCISION.

By Jabez Hogg, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital.

In placing before the Medical Society of London the results of thirty-three extractions by the semilunar corneal incision, and the experience of my last year's operations, I have been mainly influenced by a desire to show that this well known method of extraction has in no way lost its hold upon the Surgery of the Royal Westminster Ophthalmic Hospital; neither has it forfeited the confidence it inspired before the introduction of alleged modifications and improvements, which we were told would supersede the old flap operation, and also that of more modern date, lineal corneal section, both of which were so successfully practised by Adams, Travers, Ware, Gibson, Guthrie, Mackenzie, and others equally well known in this department of Surgery. Indeed, what Mr. Travers wrote of semilunar section some fifty years ago is quite in accordance with my view of it at the present time. He says:—"This operation is by far the most perfect ever devised for the cure of cataract, but it is one of considerable difficulty, and the several modifications which have been at various times suggested owe their origin to the disappointments and defects which operators meet with in learning to execute it with success."

Soon after iridectomy was fairly established in the practice of eye-Surgery, it was proposed to engraft it upon all the operations for extraction of cataract, and this was followed by a

(so-called) modified linear extraction, made by cutting out through the sclerotic, excising a portion of iris, and assisting the delivery of the lens by the aid of the hook or scoop. By such means it was said that the percentage of our failures would be considerably lessened. I am not so sure that published statistics bear out the assertion. Professor Hasner's statistics of, I believe, some 2000 cases give an average of 10 per cent. of failures. I believe that if the statistics of the flap operation in this country were carefully collected and compiled, it would give the same percentage, and not, as has been stated, 20 per cent. But whichever way this may ultimately prove to be, it is pleasant to find that the feverish stage of excitement which ushered in iridectomy has entirely passed away, leaving us free to discuss it and judge of it solely on its merits, which are no longer denied in certain cases. In our profession, experience goes a very long way in toning down the most ardent and enthusiastic admirer of any particular operation or mode of treatment; and, however obstinately blind we may be to a weak point, time generally induces a more generous estimate of opinions thus gleaned. I am about to offer you my own experience on the flap operation, but I shall take care not to weary you by a too lengthy description of my cases, or mode of operating, which, in truth, differs very little from that with which, perhaps, most of you are perfectly familiar. Neither do I intend to make an extended comparison between it and other methods practised. I shall rather confine my remarks to the results of operations performed at the Royal Westminster Ophthalmic Hospital, between April and November of 1870.

It is pretty well known that the mode of performing the operation for the extraction of cataract, like every other in surgery, has undergone a variety of changes. The instruments also have received various modifications in form and character. Nevertheless, some of the earliest known modes of operating still maintain a place in our *ars chirurgicus*. The well-known couching (needle operation), described by Celsus, and which was in vogue long before his day, is still largely practised in India and other Eastern countries. The removal of a cataract by extraction is of much later date, and was first regularly introduced by Daviel, a French surgeon, in 1747. In his excellent essay on "A New Method of Treating Cataract by Extraction," he acknowledges that he "caught the idea from Petit, who in 1708 opened the cornea to extract an opaque lens, which, having re-ascended after depression, had fallen into the anterior chamber, and that he felt himself urged to devise some new mode of operating by the want of success which he found to attend couching, and the destruction of the internal textures of the eye disclosed upon dissecting the eyes of those who had been operated upon in that way."

Daviel commenced his operation for extraction by passing a small, narrow, lance-shaped knife into the anterior chamber at the junction of the cornea and sclerotic. Curiously enough, Von Gräefe's modified linear extraction is commenced in the same way, and with a sharp, narrow-bladed knife; so that we are asked to return to a knife, and, I may almost say, an operation, which surgeons of great practical experience long since determined to abandon, for that more perfect knife introduced by Von Beer, and which seems to most of us so well adapted for the corneal incision.

The successes of those who have preceded me in this field of surgery point to the brilliancy of the results obtained by semilunar section pure and simple; and, so far as my observation enables me to offer an opinion, I feel certain that, in cases free from local or constitutional complications, extraction by the old flap operation is to be preferred to any other. Moreover, it is the only one which secures a circular movable pupil to the patient—a matter of some moment—as in this way a limited power of accommodation for viewing distant objects is preserved; while, in all the more modern operations, preceded by an iridectomy, the pupil becomes fixed, and accommodation is very nearly destroyed. Extraction by semi-circular corneal section, then, possesses the advantage of rendering the sight clearer and quicker than most other methods, while its disadvantages are comparatively unimportant. One of these, particularly dwelt upon by those who do not follow this plan of operating, is that with a large semicircular section we are very likely to have an escape of vitreous. My answer is, that it is not always necessary to make a large corneal section; it should be made of a sufficient size to admit of an easy delivery of the lens, and not larger. The probable size of the lens can generally be fairly estimated by fully dilating the pupil before attempting the operation.

Dilatation serves another useful purpose: it lessens the danger of wounding the iris, and it renders the capsule more accessible, while it facilitates the delivery of the lens. I look upon the instillation of a drop of atropine a day or two before the intended operation as a precautionary measure—in another way—that of determining suspected adhesions; it, therefore, greatly assists a diagnosis, and clears up a doubt as to which operation will be the safest, or attended with the best possible results. A patient having once had a rheumatic attack, is very likely indeed to suffer from rheumatic iritis after the most carefully performed operation. An iridectomy in such a case will often be attended with a greater amount of success than the simple flap operation. In such instances, I do not scruple to resort to the two operations at one sitting rather than follow

the practice of allowing a week to elapse between the iridectomy and the extraction.

In so much, then, my cases may be said to be selected; but this remark applies only in a very restricted sense, for it would be extremely difficult to say so when we have a frequent complication to deal with, a fluid vitreous, which might escape detection in any examination. By a careful inquiry into the previous history of the patient, we avoid the risk of failure from diabetic and other causes. I lately had a patient sent up from the country for operation with a fully formed cataract in the right eye, and the sight of the left much diminished. Upon making the usual inquiries into his general health, I discovered —what had previously escaped observation—that he was passing a considerable quantity of sugar in his urine. The opacity was due to the diabetic state of the patient, and any kind of modified operation would, in all probability, fail to restore vision.

In my thirty-three operations, three total failures occurred; these were due to causes over which it may be fairly said I had no control. In the case of Elizabeth H., admitted to the Westminster Ophthalmic Hospital, July 20, 1870, the house-surgeon reports:—"The operation, by the usual flap method, was perfectly successful, and everything went on exactly as could be wished until the third day, when the patient accidentally struck her eye rather violently, which caused considerable pain. Belladonna was applied and warm fomentations. The next morning there appeared a suspicious swelling of the lids, and on making a more careful examination, the corneal flap was found turned down. Inflammation ensued, and ultimately sloughing of the cornea. In the second case, a nervous restless patient uncovered the eye, and attempted to use it on the second day after the operation; inflammation and separation of the flap followed, and the eye was lost. In the third case, failure arose from an extremely fluid vitreous, the greater part of which escaped during the operation, causing collapse of the eyeball, and probably tearing the retina, as on the third day erysipelatous chemosis of the lids and deep-seated inflammation supervened, and the eye was lost.

It by no means follows that a large loss of the vitreous during the operation will be attended by such results as those just narrated, as I shall show you in the case of Maria C., admitted to the Hospital November 16. She was nervous about the operation, and, therefore, wished to have chloroform administered, and it was not until after a considerable quantity of the anæsthetic had been inhaled that she became unconscious and quiet enough to permit me to attempt the section. Having completed this, I was about to rupture the capsule, when she

gave a plunge, and out popped the lens, followed by a considerable quantity of vitreous humour, and a prolapse of the iris. As, from the restlessness of the patient, the iris could not be returned, I snipped it off. On the following day the patient complained of much pain; this was relieved by the administration of full doses of opium. On the fourth day the bandage was removed, and the eye open, but as the patient complained of intolerance of light, it was reapplied. A few days later an examination was made, and the eye was found to be going on well, the pupil being quite clear, and she could count fingers. At the end of six weeks, with a convex glass of two and a half inches focus, she was able to read ordinary-sized print with comfort.

I may observe that I prefer, for two or three reasons, to operate without chloroform. In the first place, the natural resistance of the recti muscles gives firmness to the eyeball, and the corneal section is made with a rapidity and cleanness, if I may so express it, quite unattainable when an anæsthetic is administered. Sickness often follows the use of chloroform, and then the vitreous either escapes, or rupture of the hyaloid membrane or of the internal vessels produces hemorrhage, and seriously compromises the operation. The risk of sickness is, however, lessened by the use of bichloride of methylene.

I look upon loss of the vitreous as a much less serious affair than some operators do. I have repeatedly noticed that the eyeball has been restored to its normal size, after the loss of a considerable quantity, in twenty-four hours, and as complete a recovery made, with a good pupil, in an ordinary period of time, as if no such accident had happened.

Adhesions of the iris are not unfrequently the cause of an escape of vitreous, and, if we fail in detecting them, the operation is not only rendered more difficult, but the convalescence of the patient is liable to be retarded.

This was the case with a patient, aged 52, in whom I met with the additional difficulty of an unusually large lens, and attachments to the iris at its inferior border. Our house-surgeon reports of this case as follows:—

"Ellen H., double cataracts. On July 18, Mr. Hogg extracted a very large hard lens from the right eye. In attempting to deliver the lens, extensive adhesions were found to exist, and, on careful pressure being made, a large quantity of the vitreous humour escaped. With some difficulty the lens was scooped out.

"July 19. The patient complains of great pain, to relieve which I gave twenty minims of nepenthe, which in half an hour afforded considerable relief. The eyelid is a little swollen, but no tenderness of the eyeball. Ammonia and bark mixture

prescribed three times a day, and four ounces of brandy in the twenty-four hours.

"25th. Gradually improving.

"August 5. With a two and a half inch convex glass, can read ordinary print."

[The following are a few remarks by Mr. Hogg on his mode of performing the operation.]

The upper section has always been preferred at the Royal Westminster Ophthalmic Hospital, and very justly so. As I have already intimated, it possesses certain important advantages over the lower section—that of less easily permitting escape of the vitreous, while the support afforded by the lid certainly favours healing of the flap. Should irregularity of the pupil occur, either from unfavourable healing of the cicatrix or injury to the iris, it is concealed by the upper lid. After the operation, separation of the section and prolapse are less likely to take place from a fit of coughing or vomiting. If the patient, however, is unable to fix the eye in the proper position, and a real difficulty presents itself in attempting the section, by reason of the eyeball rolling upwards and inwards, this is readily overcome by directing your assistant to seize, with a pair of fine-toothed forceps, a fold of the conjunctiva, and gently retain it in position. Everything considered, although upper section may be rather more difficult of execution, this is counterbalanced by its greater advantages. Another difficulty will occasionally occur, and one less easily overcome. In attempting the section, the knife penetrates the cornea too obliquely; the point then traverses the lamellæ for some distance without at all entering the anterior chamber. In such a case, the knife must be withdrawn and re-entered, or an opening must be made from above downwards with a broad-shouldered triangular knife. Such an accident will be avoided if the knife is fairly poised between the two forefingers and thumb, and the point made to enter perpendicularly to the cornea and within it, at the distance of a line from the sclerotic coat. When the point has penetrated the anterior chamber, which is known by its brightness as distinguished from the dull appearance of that part of the instrument still remaining within the density of the cornea, and by the absence of resistance which is readily felt by the practised fingers, the handle is directed slightly towards the temple, so as to give the edge such a direction when drawn in a right line that the point shall come out at the part of the circumference of the cornea exactly opposite to the point entered, thus causing the knife to divide the cornea parallel to its edge, and within a line or two of the sclerotic. It is, I consider, a matter of considerable importance that the section

should be purely corneal, and therefore it must not be carried low enough to merge into the sclerotic; a corneal margin should be left of a sufficient breadth for union.

The main impediment to success is, as I have already intimated, a section too small to admit of an easy delivery of the lens. The easy extraction of a cataractous lens, like the easy extraction of a stone in the bladder, depends upon a clean cut and a fair-sized opening; while a difficult and forcible removal is almost as certain to end disastrously. The enlargement of the section after the escape of the aqueous is dangerous to the iris and other membranes, often provocative of laceration of the hyaloid and a loss of vitreous. Should the lens sink down behind the iris to rise up in judgment against us, prolapse of this membrane is very likely to follow, and it becomes entangled in the lips of the wound. Any attempt to excise the iris after the lens has escaped will be followed by bleeding into the anterior chamber, causing not only protracted healing, but opacity of a portion of the cornea, and a dense secondary cataract from the combined effects of retained portions of capsule and serous exudation. When all goes well, very gentle pressure exerted on the eyeball, after rupture of the capsule, serves to deliver the lens, and the operation is complete. Before applying the bandage, it is necessary to separate the eyelids to ascertain if the pupil be perfectly clear. Should any portions of capsule be seen, these must be removed with a curette or scoop, care being taken not to use enough force to rupture the hyaloid, which, it should be remembered, is now bulging forward, and may very easily be torn.

By way of securing more perfect adaptation of the edges of the cornea, I take a very soft piece of sponge, and make a few circular movements over the closed eyelid, and, lastly, apply a pad of carded wool and a firm bandage. At one time it was my practice to close the lids with a narrow band of black sticking-plaster; but as this sometimes led to unpleasant consequences when there was occasion to remove it, I abandoned it for cotton-wool. The plaster is still preferred by some operators, and it certainly possesses the advantage of maintaining perfect adaptation of the lids, and secures the eye from the meddlesome interference of nurse and patient. I generally prefer to keep my patient in bed from four to six days; at the end of this period, if no inflammatory symptom appears, the bandage is partially removed, and he is allowed to move about. After the first forty-eight hours, little or no restriction is placed upon the diet, and the average duration in Hospital is under twenty days.

The construction of instruments employed in this, as in most other operations, is a point which everyone must be permitted

to decide for himself. The knife known as Beer's is that more generally used in our Hospital. For my own part, I think it a little too broad towards the base; the angle it subtends is too great. I therefore employ a modified form of this knife, in shape somewhat between a Beer and Wenzel. It is, as you will see, rather smaller, but very well adapted to fill the wound it inflicts, which is a point of no little importance in making the corneal section, as it prevents the too hasty escape of the aqueous.—*Medical Times and Gazette, May* 27 *and June* 3, 1871, *pp.* 600, 630.

54.—TREATMENT OF GRANULAR OPHTHALMIA BY THE LOCAL USE OF QUININE.

By C. Bader, Esq., Ophthalmic Assistant-Surgeon to Guy's Hospital.

The unsatisfactory results of the treatment of granular ophthalmia, especially if combined with pannus (vascular cornea), will, I hope, justify the publication of the following remarks. Experiments were made, with various substances, on patients suffering from granular ophthalmia with pannus. The idea which led to the selection of the substances experimented with was, that granular ophthalmia is the result of some extraneous substance becoming lodged in the conjunctiva, and giving rise to what have been called granulations. Experiments made elsewhere with antiseptics on organic matter have shown quinine to be one of the most effective antiseptics.

The substances experimented with on granular ophthalmia were the scrapings and juice of the root of bryonia nigra, the nitric oxide of mercury ointment (hydrarg. nitrico-oxidi, gr. iii., adipis ʒ i.) and the bisulphate of quinine in powder. The remedies were applied morning and evening; of the quinine about as much as would go on the point of a penknife was placed, with a dry camel's-hair brush, on the inner surface of each lower eyelid. No other treatment, such as exclusion from light, use of lotions, &c., was made use of. The hypothesis that quinine acts specifically upon granulations, as mercury, for instance, does on lice, may be erroneous; the effect may merely be that of an irritant, causing moderate suppuration, and, with it, removal of the granulations.

The effects on granular conjunctiva of bryonia nigra and of the nitric oxide of mercury ointment, though curious in other respects, are of little interest here. Suffice it to state those of quinine. In some cases its application was followed by severe

smarting, which continued for ten or fifteen minutes; in other cases no pain whatever was felt; in all cases appeared increased purulent discharge from the conjunctiva, with shrinking of the granulations, and clearing of the surface of the cornea. The intolerance of light ceased rapidly in all cases; the dilatation of the pupils appeared in from twelve to twenty-four hours after the first application of quinine. The pupils, though dilated in ordinary light, contracted well on exposure to strong light.

Case 1.—A. B., aged fourteen. Granular ophthalmia for six years; at present vascular cornea (pannus) in both eyes; pupils barely visible; granular conjunctiva; extreme intolerance of light. The application of bryonia nigra was followed by symptoms of purulent ophthalmia (chemosis, swollen lids, &c.), which, with slight improvement of sight, subsided within ten days; the intolerance of light persisted. Quinine was used and the bryonia discontinued. The eyes improved rapidly; the granulations became smaller, and the intolerance of light disappeared completely within four days.

Case 2.—C. D., aged ten. Granular ophthalmia with pannus for two years, with intolerance of light. The use of the nitric oxide of mercury ointment (for fourteen days) was followed by slight improvement. It was discontinued and quinine substituted. Within ten days all intolerance of light had ceased, the cornea became much clearer, &c.

Case 3.—E. F., aged ten. Granular ophthalmia for one year. The intolerance of light persisted for three weeks while the nitric oxide of mercury ointment was used; it disappeared on the second day after the quinine had been applied.

Case 4.—G. H., aged thirty-four. Granular ophthalmia for one year, with pannus and intolerance of light. Quinine was used for ten days; on the third day the pupils became dilated, and on the fourth all intolerance of light had ceased.

Case 5.—I. J., aged eleven. Granular ophthalmia for four months, with slight pannus and intolerance of light. Quinine treatment for three weeks. All intolerance ceased; cornea much clearer; granulations red, much smaller; slight purulent discharge.

Case 6, aged fourteen, bad for three months; *Case* 7, aged fifteen, bad for four years; *Case* 8, aged ten, bad for two years, improved equally rapidly under the quinine treatment; in all did the intolerance of light disappear within from five to ten days, with marked improvement in the state of conjunctiva and cornea.—*Lancet, Oct.* 28, 1871, *p.* 604.

55.—ON THE USE OF CROTON OIL IN OLD OPACITIES OF THE CORNEA.

By Thomas Windsor, Esq., Manchester.

During the last few years I have from time to time tried a number of substances as local applications to the eye. Some of these, as croton oil, had not, to my knowledge, been employed previously; others, as the ol. sabinæ, had already been used on the Continent. All, except croton oil, seemed to me in no way superior to the medicaments commonly ordered.

This oil, however, proved to be of service in cases of old opacity of the cornea when the ordinary remedies caused little or no improvement. I must add that I never used it, until other means, including the pure turpentine oil, were found insufficient, so that I can at present only say that when the eye bears the latter without difficulty, croton oil, diluted with sufficient ol. olivæ, is an efficacious and yet manageable stimulant. I have generally used one part of it to seven of olive oil, commencing with a weaker form: I have never applied it stronger than in the proportion of one to five. The latter has, indeed, sometimes caused slight diffuse haziness of the cornea, which disappeared again in the course of a few days. Severe pain, much lachrymation, and great redness of the eye, usually come on in a few minutes. I let the patient bathe the eyelids freely with cold water when the pain is very severe.

To exemplify its effects, I may take a case of parenchymatous corneitis of a year's standing, in which, at the time of application, the periphery of the cornea was almost perfectly free from vessels. Three minutes after, one drop (dilution $\frac{1}{7}$) had been applied, there was a pale purplish blush, about two lines in breadth, around the cornea; the posterior conjunctival vessels were much enlarged, and there was free lachrymation. Eight minutes later the limbus conjunctivæ was much injected, the surrounding purplish blush was more intense, whilst the conjunctival vessels remained much in the same state. A few minutes more, and examination by oblique illumination showed that numerous small vessels connecting the anterior and posterior conjunctival vessels were distended with blood. Two days later the congestion had entirely disappeared.—*Man. Med. and Surg. Reports, Oct.* 1871, *p.* 56.

56.—THE TREATMENT OF WOUNDS OF THE EYE BY SUTURE.

By Thomas Windsor, Esq., Manchester.

[It was in the trials which were so often made between 1820 and 1840 to transplant the cornea from one animal to another,

that surgeons first found themselves obliged to apply from one to four sutures to the eye for the purpose of accurately fixing the fresh cornea.]

Critchett, preceded however by Wilde, whose example attracted little or no notice, began about 1861 to suture the wound caused by abscission of staphyloma. When he published his paper two years later, the surgeons at Moorfields had treated thus about thirty cases, and suppuration had occurred in only four. The suture was generally left in for some weeks.

An American surgeon, Dr. Williams, has extended the use of the suture to flap extraction. He thinks that it obviates the danger of loss of vitreous after the operation, in case vomiting should occur, that it renders primary adhesion much more certain, that it removes nearly all risk of prolapse of the iris, that it renders the free use of atropine admissible, that it enables the surgeon to examine the eye at an early period with safety, and that it shortens the period of convalescence. He uses a single strand of the finest silk and a fine needle, a quarter of an inch long, with a flat cutting point. The needle is held by strong forceps, and passed as near as possible to the edges of the wound, which are held by very delicate forceps. Latterly he seems inclined to change his method a little; in a paper on this subject published in 1869, he remarks that, "at present I am disposed to extend the corneal flap at its apex a little way into the conjunctiva, so as to allow of the placing of the suture in this membrane, where it is more easily inserted than through the tougher corneal tissue." The suture, if left to itself, as was at first his usual practice, generally comes away in a few days; he thinks it, however, best to remove it in a week or ten days, though in more than one case it has remained for seven weeks without giving rise to any but trivial irritation. In no case—and in 1867 he states that it had been used in nearly 100 cases,—did it do harm.

The only case in which a wound of the sclera had been sutured was, so far as I knew when the following case was under my care, one recorded by O. D. Pomeroy, though I have since met with two in Mr. Lawson's book, and another has lately been published. To these I shall again refer. I may add that Von Walther just mentions the use of sutures in wounds of the eye; he dissuades from their use, though he makes no mention of ever having tried them. Pomeroy's case was as follows:—

"John S. N., aged 21 years, a soda-water manufacturer, was struck on the eye by fragments of a soda bottle which had burst, inflicting a wound of the sclerotic at the margin of the cornea, about four lines in length, causing a prolapsus of iris (and perhaps of choroid,) an unsightly coloboma resulting. I

easily succeeded in reducing the prolapsus, although it remained but momentarily. Two sutures were then passed through the conjunctiva, near the margin of the wound, and gradually tightened as the prolapsus was reduced; they failed, however, in reducing the whole, so a suture was placed between the two already in. The sclerotic aspect of the wound afforded a sufficiency of conjunctiva, but not so with the corneal; so the suture was passed through a portion of the sclerotic at its corneal junction, which closed the wound satisfactorily. The after-treatment consisted in the application of ice to the eye, atropine, and leeches. In fifty hours the lateral sutures were removed, when the wound was found to be well adhered. On the next day an effort was made to remove the central suture, but as the wound showed signs of gaping if much interfered with, it was not removed until the following day, when the wound was found to be well united. There was a little conjunctival injection, which disappeared in a few days. Patient sees as well, or nearly so, as with the other eye, which is normal."

This case came at once to my recollection when, on August 4th, 1870. I was called at the recommendation of Dr. Bower Harrison to see a child whose left eye had been injured two or three hours previously. The account given was, that whilst an elder brother was cutting twigs with a gardener's curved knife, large aud blunt, the injured boy happened to stoop down, the knife slipped and was driven into his eye.

On my arrival I found a delicate-looking boy, very frightened and inclined to cry. Gently raising the upper lid, I found an incised wound, commencing in the inner and upper side of the cornea and extending through the sclera backwards for fully half an inch; there was a nodule of prolapsed iris in the anterior part; the rest of the wound was gaping widely, and through it the vitreous was bulging, though none appeared to escape.

The question of a ligature at once arose. It appeared to me, however, inadvisable to give chloroform, because the child was certain to cry and struggle, and the least effort would probably force a quantity of vitreous through so large a wound. I therefore decided to wait. A bandage was applied so as to gently compress the injured eye, and the other one was closed by plaster. He was kept in bed some time; hardly a trace of inflammation occurred, and the wound gradually contracted.

The progress being slow, I decided to remove the prolapsed iris. For this purpose Dr. Finlayson gave chloroform on August 17th, and I excised the nodule. This had a beneficial effect; the anterior part of the wound was soon firmly healed, the posterior part uniting also for a little distance further. In Novem-

ber the centre was still open and allowed the vitreous to project; the bandage was now tightly applied and continued till December 27th, when as the prolapse of vitreous rather increased than diminished, Mr. Fletcher giving chloroform, I excised the projecting nodule and passed a suture through the middle of the wound. As the edges of the sclera seemed to be thickened, I endeavoured to pass the thread obliquely through it, after the manner of suturing a vesico-vaginal fistula. The stitch was then drawn tight and completely closed the wound. The eye was kept at rest by a bandage. Next day there was considerable injection of the parts immediately around the wound, accompanied by some pain. On the 30th the congestion was less, and the suture had cut partially through.

A day or two later the suture came away spontaneously, and on January 6th the wound appeared completely closed. On the 20th the cicatrix was quite level, and all irritation had disappeared.

My last note is, "February 24th—Wound appears firmly healed. Pupil of good size, but a little displaced inwards, with a rather large coloboma extending to the margin at the inner and upper side; it reacts moderately to light. He reads slowly No. 5. and letters of No. 4 (Jäger.) The lens is not displaced. It, the other media, and fundus are normal."

I directed that the bandage should be gradually discontinued, and have not seen him since. The other eye remained throughout normal.

Mr. Bowman's cases were equally satisfactory. In the first one, John M., aged 25, was admitted into the hospital, October 31st, 1863, on account of an injury of the left eye, received three days previously. When admitted, there was a horizontal wound in the sclerotic at the inner side of the cornea, about one-sixth of an inch in length. The edges were not in apposition, and through it vitreous was oozing. The anterior chamber was excessively deep; the tension of the globe was—T^3 and was daily diminishing. He could count fingers at from six to seven feet.

Mr. Bowman performed iridectomy at the inner side, and then the edges of the wound in the sclerotic were brought together by a very fine suture, the needle being made to pass through each lip of the wound from within outwards.

The man had a good deal of pain the second night after the operation, but it was relieved by the application of a leech to the temple. On November 4th the iris had resumed its proper place; the tension was normal. The thread had come away. He could see letters of 16 Jäger's types. He left the hospital November 25th, and could then read letters of No. 10.

Mr. Lawson gives the following account of the second case: —" Mary S., aged 9 months, was brought to the hospital on July 7th, 1863, suffering from an injury to the left eye. A piece of china in falling off a shelf struck the eye, and inflicted a small wound in the sclerotic, close to the corneal margin. The wound (notwithstanding that a week had passed since the accident had occurred) was still a gaping one; the edges of it not being in apposition. Mr. Bowman applied a single suture, and with it brought the lips of the wound accurately together. No irritation followed; and on July 21 the child ceased its attendance at the hospital, the eye being quite well."

Mr. Lawson has lately published a case that had been under his own care. W. G., aged 40, was admitted on March 5th, 1869. There was a wound in the sclerotic at the lower part of the left eye, a short distance from the cornea; this had been caused by a blow of a stone. A small quantity of vitreous had evidently escaped. The wound gaped; its lower edge was prominent, and stood away from the upper margin, which was somewhat depressed. A single thread brought the parts into apposition. The patient progressed most favourably, and the wound completely united; the stitch produced no irritation. On April 27th there was no redness of the eye, which seemed to have recovered completely; he could, however, only count fingers. An extensive detachment of the retina was found to correspond with the site of the puncture.

These cases require no lengthy commentary; the effect of the suture has been in them too palpable to leave room for doubt. I think it is clear that whenever a wound of the sclera or cornea gapes, a suture should be applied, unless there is some special contra-indication. Chloroform should be generally employed, and the thread should be very delicate. If it causes little or no irritation, it will be usually the best plan to wait for its spontaneous discharge.

The principal objections to the suture are—the frequent necessity of chloroform for its insertion, and possibly for its removal; the difficulty of inserting it; and the risk of its exciting inflammation. The latter appears to be very slight, judging by the cases hitherto recorded.

I may conclude with the suggestion that, as union at once followed in my case, though the edges of the wound had been separated for many weeks, similar treatment would not be unlikely to have a similar effect in fistulæ of the cornea and sclerotic, which it has been hitherto found to be so difficult to close.—*Manchester Medical and Surgical Reports, Oct.* 1871, *p.* 3.

DISEASES OF THE SKIN.

57.—ON AURICULAR HERPES ZOSTER AND ITS TREATMENT.

By Dr. F. E. Anstie, Senior-Assistant Physician to the Westminster Hospital.

[During the week or two preceding the writing of this paper, Dr. Anstie had been the subject of herpes zoster in an unusual form, and the apparent results of treatment involved a therapeutic fallacy, which it is instructive to investigate. For about twenty-three years, Dr. Anstie has been subject to attacks of neuralgia in the right trigeminus, the severity of which however has of late years been much diminished.]

On August 24th I began to suffer most acute pain in front of the tragus of the right ear, which came and went several times in the day, and which darted from the place where it commenced into the meatus auditorius and the maxillary joint, and ran up the side of the head. I concluded that I had caught cold from unusual exposure to draughts in a house that was undergoing repairs, and at first was not certain that there would not be abscess of the meatus; but in the course of forty-eight hours it became quite obvious that that was not the nature of the malady, for the pains recurred with great regularity four times in each twenty-four hours, and in the intervals the parts were quite free from pain; besides, they could be handled, at any time, without showing tenderness. On the 29th the pains became so violent as to necessitate the use of morphia whenever they recurred, and had they been merely superficial I should have endeavoured to stop them (as I have repeatedly stopped my attacks of frontal neuralgia) with the constant current: but I shrank from the application of the constant current deeply within the meatus (which must have been done), from an uncertainty as to its physiological effects upon the auditory nerve. On this, the sixth day of the neuralgia, I begged Mr. Carter to examine the ear with the speculum; he found the meatus and the membrana tympani perfectly healthy, without redness or tumefaction. The pain now was at its acme of severity, and the paroxysms extended not only into the branches of the auriculo temporal, but into the labial branches of the inferior dental nerve, and passed over into the superficial cervical nerves, running quite down to the right shoulder. The intractability of the affection puzzled me greatly. On the 31st August Mr. Carter, examining the meatus again, reported that there was now some slight internal tumefaction and redness; but it seemed difficult to say if this were not partly caused by local

applications which I had used, in desperation, during the attacks. I now requested Mr. Hinton to examine the ear; he did so, and verified the moderate tumefaction, and thought he detected a very small "pimple," but believed that it was merely the effect of local applications, and that the case was one of pure neuralgia. Mr. Hinton advised quinine, the use of soothing unirritant applications, and the exclusion of air. Accordingly I treated the periodic attacks with the continuous application of hot poultices covering the entire ear and the painful point in front of the tragus (where there was, by this time, a veritable *point douloureux* of Valleix), and the use of large doses of quinine. In the course of forty-eight hours I had taken nearly sixty grains of quinine; and by the end of that time a striking amendment was produced, so striking, in fact, that it seemed impossible to avoid the inference that the quinine had caused it. (No discharge of pus whatever had taken place.) I discontinued the poultices, and on the 6th September ventured also to remove the cotton-wool with which I had plugged the ear, in the intervals of poulticing, to exclude cold; but soon found that the part was too sensitive to admit of this being done with impunity. My attention was now attracted to a few small vesicles in the central folds of the pinna, and a little group of two or three at the external edge of the cartilage; the latter, being rubbed, ulcerated and became very painful. A draught of cold air on the spot would momentarily set up the old neuralgic pain. At the same time a small group of characteristic herpetic vesicles formed below the right angle of the mouth, and invaded the mucous membrane; unluckily, in forgetfulness, I irritated these by smoking a pipe on that side of my mouth, and they coalesced to form one rather nasty ulcerated pustule. It was only on September 11th that I discovered that there had also been a few vesicles (then dried up) on the neck, over the right sterno-mastoid; though I had noticed some soreness in washing and wiping the part. By the 13th the vesicles had everywhere completely disappeared, and all tendency to neuralgic pain had also apparently left me.

The first remark on this narrative that occurs to me (beyond the reflection that had the case been anyone else's than one's own, one would perhaps have discovered its nature much sooner), is that the apparent curative influence of the quinine was probably altogether delusive. The time during which it was apparently producing such marked effects was evidently that during which the herpetic eruption was beginning to come out, and that is, I need not say, a period at which the violent pain of neuralgic herpes often ceases: in fact, in the majority of cases that I have seen, the pain was then at least temporarily suspended. To say truth, the decidedly beneficial

action quinine had seemed to exert had greatly surprised me, for a long experience, especially in the treatment of my own case, had led me to an utter distrust in quinine for non-malarial neuralgias; and I had even begun to speculate whether, in a recent short trip to Westmoreland, I had not caught a slight taint of malaria, from which the neuralgia had resulted. I think this idea may now be entirely set aside, and the explanation adopted that the relief of the pain was altogether spontaneous.

The true treatment for such a case seems to me not at all doubtful, since it is the same which I invariably adopt with success in ordinary shingles attended with neuralgia. Morphia should be given hypodermically, $\frac{1}{6}$ grain twice a day; local injection (near the trunk of the auriculo-temporal nerve) is for once to be preferred; and at the same time the painful parts should be hermetically sealed from the air. For the external parts collodion flexile constantly re-applied is the best thing; within the meatus, a good thick coating of simple ointment (put on melted with a brush), and an external plug of cotton-wool, should be used. The grand points are, to exclude the irritation of external cold, and to prevent the neuralgic pain from rising to any great severity: with this treatment I believe that zoster may always be kept within bounds, and the vesicular eruption prevented from developing to any large extent. And, what is very important, the vesicles are prevented from becoming open ulcers, a condition which is almost certain to involve a troublesome revival of the neuralgic pain.

The other remark to which the above related history gives rise is pathological, and has only an indirect bearing on treatment, but I may be excused for introducing it, because the rarity of the case makes it important not to lose any lessons that it may suggest. I say the *rarity* of the case—but my very point is, that such cases are perhaps not so uncommon, there being a possibility, and even likelihood, that the majority of them simply escape recognition. The idea that has occurred to me is this: that a large proportion of cases of earache, which are generally set down as examples of suppurative inflammation of the meatus auditorious, are in reality *nothing but neuralgic herpes, depending on an affection of the auriculo-temporal nerve*. What is the history of common earache? So far as I have seen, there are comparatively few cases in which the pain begins very gradually and then continuously increases up to a moment at which pus is discharged and immediate relief obtained; yet that should be the invariable order of events in the case of suppurative inflammation of the lining of a narrow bony canal like the meatus auditorious. Inflammations occurring in organs where

there is room for expansion, may, and usually do, cease to be actively painful during the middle stages of the process, and only become painful again (with a throbbing agony) when pus is actually forming. But in a confined space like that within the meatus auditorius, the presence of pain is continuous, since the products of inflammation very early begin to produce severe tension. But in ordinary earache, for instance that to which children are liable, the course of events is, I believe, quite different from this. The pain often comes on in the first instance violently; the patient cries out with the severity of the suffering; but then there are remissions, amounting often to complete intermission. Moreover, so far as I have seen, it is but rarely that a well-defined discharge of pus is observed, even when relief to the pain has been somewhat suddenly obtained, as is not unfrequently the case. Indeed I have often been exceedingly puzzled by my entire failure to detect more than a very moderate amount of tumefaction within the meatus, and by the fact that the closest observation could not find a single globule of pus on the poultice or other dressing applied to the ear at the time when the pain became suddenly mitigated. On the contrary, there very frequently is found, what I observed in my own case now related, a shedding of small scales, either dry or moistened with a very little bloody sanies, from the lining of the meatus, a few days after the cessation of the pain: in fact, what looks exactly like the dead epidermis of vesicles that have faded. The whole course of events seems to correspond much more nearly with that of auricular neuralgic herpes, than with that of common abscess of the lining membrane of the meatus.

Should this suggestion prove correct, and should it be established that a neuralgic auricular herpes is the cause of a considerable proportion of cases of earache that are commonly supposed to be suppurative on account of the sudden way in which relief is at last obtained, some very interesting further considerations will be opened up. Earache is comparatively common among children, whereas the ordinary forms of neuralgia are exceedingly rare, before puberty. But on the other hand, shingles attacks children as frequently as adults; a very common age for it is between 8 and 10. Ordinary unilateral herpes is, however, not attended by actual neuralgic pain, in children; although there are highly disagreeable sensations (especially previous to the outbreak of the eruption) resembling, for the most part, very hot water running about underneath the skin. Why should *auricular* shingles, alone, be attended with true neuralgia in children? as would be the case on the hypothesis which I have put forward. This is, no doubt, a difficulty, and for the moment I see no answer to it; but the

mere circumstance that there is a difficulty in understanding why so uncommon a phenomenon as true neuralgia should attend this one form of herpes in children, does not alter the fact as to which I am confident, that in a vast number of cases of earache, in children as well as in adults, the pain is intermittent in character, and there is never any evidence of that discharge of pus which is presumed to account for the sudden departure of the pain. Under these circumstances, what seems like an anomaly ought only to serve us as a starting point for fresh inquiries which may perhaps lead us to interesting results; and I beg to throw out one suggestion that may be valuable. What is the meaning of the greatly higher irritability of the inferior maxillary (of which the auriculo-temporal is a branch) and of the superior maxillary, than of the ophthalmic division of the fifth nerve in childhood? for that this higher degree of irritability exists is an unmistakeable fact. How exceedingly rare is neuralgic headache before puberty; and how very common, *in children of a nervous temperament*, is toothache, from a very slight amount of caries, assuming all the intensity of a severe neuralgia, and darting into the ear, the throat, and the cervical nerves! I think it may be unhesitatingly said that the superior maxillary and the inferior maxillary nerves are the first of all sensory nerves in the body to exhibit any tendency to pain of a grade approaching the severity of true neuralgia: and I can only suggest that the reason of this is, possibly, the depressing and exhausting influence exerted upon them by the growth of the teeth. But in truth we are as yet only just beginning to see the great importance of the questions that are connected with the comparative dates, in the physiological life of individual portions of the nervous system, at which those portions respectively exhibit certain phenomena, such as the tendency to pain, or convulsive action, &c.

Meantime, if my suggestion—that ordinary earache may be, in a considerable number of cases, merely auricular herpes zoster—be correct, it ought to produce certain practical consequences by which it can be partly tested. It must surely be unnecessary, or even hurtful, to apply leeches: a business which is always tedious and uncomfortable, and which (when the application is to the mastoid) sometimes leads to an amount of loss of blood which a delicate child can very ill bear. The only remedy of any value beyond the hypodermic injection of morphia, and the sedulous exclusion of air from the skin of the whole affected nerve-territory, would be distal or reflex stimulation, by the application of mustard or cantharides over the branches of the great occipital nerve, in the "occipital triangle" of anatomists. There is reason to think that this might some-

times mitigate both the acute pain and the tendency to extensive vesiculation. But it would be necessary to avoid making a very severe impression, for in the latter case there would be some danger of setting up erysipelatous inflammation of a large tract of the neck, face, and scalp; a consequence which I once saw follow a too violent and extensive blistering of the nape for the relief of supra-orbital neuralgia.—*Practitioner, October* 1871, *p.* 198.

58.—ON THE TREATMENT OF GANGLION AT THE LONDON HOSPITALS.

The treatment of the common affection called ganglion, simple as it may appear and is generally supposed to be, is still a subject on which a very considerable difference of opinion exists amongst surgeons, as may be seen by a perusal of the following notes. It was with the view of gathering the opinions of hospital surgeons on the various methods of treating this affection, that we thought of collecting a few notes on the subject. We have been by their kindness enabled to obtain a mass of valuable information, containing the experience of most of the London hospital surgeons; and shall endeavour to procure similar notes from provincial hospitals and from Scotland and Ireland. We shall be glad to receive at the same time short notes of interesting cases of ganglion, for publication, in connection with our report on the general treatment of the affection.

King's College Hospital.—Mr. John Wood treats ganglion by the following plan:—A spear-pointed needle, cutting on both edges, and mounted on a handle, is passed into the cyst, and made to transfix it again and again, so as to let out the synovial contents into the areolar tissue of the surrounding fascia. The needle is then made to scarify briskly the interior of the cyst, and is used pretty freely in dividing the cyst-wall at its opening of communication with the sheath of the tendon. Pressure is then made with both thumbs upon the tumour, so as to squeeze out completely its contents, partly into the subcutaneous areolar tissue, and partly out through the opening in the skin by which the needle entered. Iodine paint is then applied thickly over the surface, and upon it a thick pad of lint, over which firm pressure is made by a bandage. This is kept on for several days, after which the iodine paint is again applied, and the pressure readjusted. After a few applications in this way, the tumour seldom reappears; and, if it do so, a repetition of the process rarely fails to succeed. No case has been met with, out of many hundreds treated, in which suppuration or any bad results have followed this plan; but several

cases in which a seton had been employed have given rise to much trouble and danger from erysipelatous inflammation and abscesses, followed by stiffening, and in some cases permanent impairment of the use of the limb.

Mr Henry Smith meets with a large number of cases of ganglion in the out-patient department of the hospital. After having tried various means of cure, he has come to the conclusion that the most effectual is that of operation by the seton. He passes a single ligature thread through the cyst, and allows it to remain according to circumstances. In some instances, severe inflammation and even suppuration will be produced in forty-eight hours, and then the thread is to be withdrawn. In the majority of instances, however, especially when patients are careful not to use their hand, the seton may be retained for a period varying from three days to a week or more, without producing any inconvenient symptoms; but, so soon as suppuration takes place, Mr. Smith withdraws the thread, and the cure is almost invariable. It is necessary to bear in mind in this treatment, that, in some constitutions and under certain conditions, the presence of the seton may produce very severe consequences; in fact, this is the only objection to the treatment. With care this rarely occurs; and there has only been one instance amongst Mr. Smith's patients at the hospital where bad results did happen. This was in the case of an unhealthy man, who applied, with a ganglion as large as a crown-piece on the back of the wrist. Mr. Smith passed a seton. The patient did not apply until after four days, and in the meantime most violent inflammation and suppuration occurred. Free incisions were necessary, and the wrist-joint itself was threatened for a time; but the use of a splint, and careful treatment, prevented any mischief. The patient, however, was compelled to remain under treatment for several weeks.

Mr. Royes Bell often finds that a ganglion is too tough to be burst by any reasonable amount of external pressure, or is so situated that this form of treatment is not applicable; and that it may be radically cured by a puncture with a grooved needle, squeezing out the contents as perfectly as possible, and rubbing the sides of the cyst together. Firm pressure must be applied by means of a pad formed of lead, gutta percha, or a piece of money, tightly strapped and bandaged over the site of the ganglion. Should this treatment not succeed, he prefers lacerating the walls of the ganglion subcutaneously with a strong needle with cutting edges, to the use of the seton. He has seen many excellent results follow this method, which is less severe than the seton, though the latter no doubt is the more radical form of treatment. He has seen a severe case of

ganglion connected with the flexor tendons of the wrist, and passing with them under the annular ligament, cured by laceration. Mr. Bell has also evacuated a ganglion by means of an incision, squeezing it roughly, so as to get rid of all its contents. In this case, the ganglion returned. These swellings have a tendency to recur; and in weakly persons, after a radical cure of one, another and a fresh one may form. For ganglia which resist these simpler plans of treatment, the seton remains, which Mr. Bell thinks less manageable than the other methods. In passing a seton through a ganglion at the back of the wrist in an out-patient, he advises the hand and wrist to be placed on a straight splint for the time being. In the severe form, affecting the flexor tendons under the annular ligament, the patient ought to be in hospital, under constant observation and control, as out-patients are proverbially careless. The after-treatment, by carefully-applied pressure, to be kept up for several weeks, is of considerable importance.

University College Hospital.—Sir HENRY THOMPSON applies, for ordinary and recently-formed ganglia about the wrist, tincture of iodine for four or six weeks, usually with good effect. If they resist this, he passes carefully through the centre, with a sharp needle, a double thread of silk; ties the two ends in a knot, squeezes out the contents by the needle-opening, and leaves the thread in for three days, applying water-dressing. At the end of that time, if a purulent discharge be seen, and a little inflammation have taken place, Sir Henry removes the thread, and applies water-dressing. Almost always, there is no more trouble with the ganglion. If little or no action be produced by means of the tiny seton, he leaves it in a day or two longer. Sir Henry has never had occasion to regret this but once. An out-patient at the hospital, who did not attend at the end of three days, returned a week after the operation with erysipelatous inflammation of the arm. She did badly, and got some permanent stiffness of the hand in consequence. That is the only unfortunate event among a great many cases which Sir Henry has thus treated; and, had he seen her at the end of the three days, he has no doubt all would have gone well.

Mr. CHRISTOPHER HEATH finds ganglion affecting the extensors of the wrist to be a common affection among the out-patients of University College Hospital, and readily amenable to treatment. If the cyst do not yield to the pressure of the thumb, steadily exerted for a minute or two, Mr. Heath is in the habit of puncturing it with a grooved needle, and evacuating the jelly-like contents. He has never seen any harm result from the practice, and finds the subsequent application of iodine paint for a few days apparently prevent the refilling of the

cyst. Ganglion of the extensor tendons on the back of the foot is by no means rare; and within the last few months Mr. Heath has seen the disease in more unusual situations—viz., deeply in the ball of the thumb, in connection apparently with the tendon of the flexor longus pollicis; and again behind the external malleolus, in connection with the peronei tendons. In both instances, a puncture settled the diagnosis and concluded the treatment simultaneously: in the latter case affording great relief to the mind of the patient, who had been assured, by a leading authority on deformities, that the tumour was fibrous, and would require a serious operation for its removal. In the compound ganglion of the flexors of the wrist, where the swelling forms above and below the annular ligament, and the fluid contains bodies like rice or melon-seeds, Mr. Heath has evacuated the fluid by a puncture, taking care to keep the hand on a splint; and when this has failed, as it generally does, to effect a cure, he has employed the seton with success.

St. Bartholomew's Hospital.—Mr. SAVORY treats those sacs filled with glairy fluid, which are so apt to form about the wrists and hands, by puncture, complete evacuation of contents, and firm, equable, and continued pressure, as described by him in the second volume of *St. Bartholomew's Hospital Reports*, p. 92. Mr. Savory treats ordinary cases of enlarged bursa over the patella and these ganglia in the same way.

Mr. THOMAS SMITH believes the best way to cure ganglion to be to rupture the cyst by forcible compression, if possible (not by a sudden blow), and to keep a pad and bandage applied for some days afterwards. Even where the ganglion does not suddenly collapse, the effect of a hard squeeze may be to cause gradual absorption. When the synovial cyst is too tough to be affected by the previous plan, he is in the habit of making a subcutaneous section of the entire cyst-wall with a fine tenotomy-knife, and keeping up pressure for some days. If, from any objection on the part of the patient, or from other causes, this cannot be done, he has occasionally cured the disease by blistering with ointment of biniodide of mercury. He has no experience of injecting, excising, or passing setons through ganglia.

Of the treatment of ganglion (as distinct from the recognised bursal swellings) at St. Bartholomew's, Mr. WILLETT says that attention must first be drawn to the frequency with which this affection is met on the dorsal aspect of the wrist and hand. For its cure, Mr. Willett relies on forcible dispersion, either by rupture of the sac by the thumbs (which, by the way, requires dexterous manipulation, rather than any great amount of force), or, this failing, by puncture with a Paget's knife. But,

whether the ganglion be ruptured or punctured, the essential element of success lies in the complete evacuation of the cyst's contents; and, to ensure this being done, tolerably rough handling in the way of pressing and squeezing the sac may be employed. Then the application of a compress of lint and roller completes the operation. Mr. Willett believes that, when dispersion fails, the reason is, that the operator has been content with simply rupturing the cyst; but, if proper care be only taken to empty the ganglion thoroughly, this plan of treatment may be regarded as almost invariably successful. With regard to the so-called ganglion affecting the sheath of flexor tendons at the wrist, Mr. Willett coincides with the opinion that, should operative measures become necessary, it is desirable to lay open the swelling above the wrist tolerably freely.

Guy's Hospital.—Mr. Poland adopts every possible variety of measure, with success and non-success. Puncturing and emptying out the contents, with subsequent pressure, has had very satisfactory results, but not always so; and when failure has resulted, he has laid open the ganglion, with a cure resulting.

The treatment of ganglion which Mr. Howse adopts varies with the nature of the cyst. If it be small and the cyst-wall thin, he thinks that forcible compression, so as to produce rupture, will give the best results. If, however, it be a large thin-walled cyst, close under the skin, without the distinct outline which such tumours generally present—tending, in fact, to become diffused—then repeated blistering nearly always gets rid of the affection. In cases of the above description, which are only slowly amenable to the blistering treatment, the cure may be much expedited by puncturing the cyst. There are, however, a certain number of cases which are not curable by any of the above methods; where the cyst-wall is thick, and not capable of being ruptured, or where it is situated under dense fascia, as in the palm of the hand. Such tumours are often complicated by the presence of a large number of "millet-seed bodies." These are, he thinks, best and most expeditiously treated by excision of the cyst in the antiseptic mode. The usual objection to this plan of treatment is the fear of diffuse inflammation supervening. The antiseptic method, however, entirely obviates this objection, and with its aid he has no fear of opening the sheaths of the tendons even extensively. In this operation, Mr. Howse has found it no objection that the whole of the cyst-wall cannot at all times be removed. In spite of its presence, the wound will generally close by adhesion, and not a drop of pus be found. Even in puncturing a ganglion he would generally adopt the antiseptic

method, considering it safer so to do. In most cases Mr. Howse prefers excision of the cyst to injection with iodine or any other irritant, having once or twice seen a good deal of inflammation set up by its means. For the same reason, treatment by setons is, he believes, not a very safe mode of procedure.

Mr. Davies-Colley's usual practice has been to disperse ganglia by pressure; and, if they are too hard to be treated in this way, to blister them.

St. Thomas's Hospital.—Mr. Le Gros Clark treats ganglion when in the wrist by puncture, subcutaneously, with a narrow lancet, or broad spear-shaped needle, and scratches the interior of the sac. He then presses out the contents beneath the skin, and subsequently applies a lead compress for a few days. This rarely fails, is almost painless, and is, in his experience, unattended with risk. When on the hamstring muscles, over the great trochanter, &c., he finds repeated blistering the safest and most efficacious treatment, or a blister kept open with iodine-ointment. Opening these large ganglia is not unattended with risk. It is safer, if opening be intended, to blister first. On the front of the wrist, and extending into the palm, blistering may be tried. Mr. Clark has not often been successful when they are large in this position. If opened, it should be by a free incision in this position. He prefers this to seton. Mr. Clark has never injected a ganglion, nor has he found it necessary to dissect one out.

Middlesex Hospital.—Mr. Campbell De Morgan prefers to leave slight cases of ganglion alone. If troublesome on the back of the wrist, he fairly cuts through them subcutaneously, and keeps on pressure. The large ones on the palm of the hand, if he interferes at all, Mr. De Morgan lays open fully, saving the annular ligament; and dresses with some balsamic tincture. Enlargements of the bursa patellæ he treats with rest and iodine. If they be inflammed and suppurating, he lays them open. When they are indolent, he uses puncture and rest, sometimes setons, though Mr. De Morgan states that he has seen mischief from these. He has seen great good from blistering. Ganglia in the popliteal space he never touches, if he can help it. If it be necessary to interfere, the greatest care should be taken to preserve rest. Inflammation is often propagated to the joint from them.

Mr. Nunn treats ganglion in the following manner:—Ganglion—that is to say, circumscribed inflammation of synovial tissue in juxtaposition with the sheaths of the tendons, forming a tense roundish tumour, more or less moveable—if not capable of being dispersed either by steady pressure or by sudden force, rupturing the sac and extravasating the contained fluid into the

surrounding cellular tissue, cannot be treated off hand, like an abscess or a boil, by puncture, without considerable risk of diffused inflammation involving the sheaths of the tendons and intermuscular cellular tissue. As ganglion is frequently caused by excessive movement of the tendons in their sheaths—as in persons training for professional pianoforte-playing—it is difficult to imagine that ganglia are so completely independent of the synovial structure lining the proper sheaths of the tendons as is generally accepted. One might suppose that a pouch could be commenced by a slight effusion of fluid in the sheath, distending the delicate true synovial membrane through a gap in the investing firmer tissue, and that this pouch could become a cyst, as in well-known parallel examples, by obliteration of neck of sack. The fact is (as far as Mr. Nunn's experience goes) that the ganglion is usually, if not invariably, found on the back of the hand. Where the sheaths of the tendons are undefined, and apertures common, Mr. Nunn understands that circumscribed effusions *within* the sheaths of flexors or extensors should not be included in the term ganglion, although in many cases, doubtless, essentially of the same nature. These swellings are, on the other hand, sometimes also due to constitutional causes. Subcutaneous puncture, or rather incision, is, in cases not to be ruptured by blow or pressure, probably the best next step, but may require repetition, and should not be done without using a splint as a precaution. The subcutaneous puncture is probably quite as safe as severe counter-irritation—a plan which Mr. Nunn has seen tried. But, before using even subcutaneous puncture, he would advise diligent trial of alternate douches of very hot and cold water: this will remove almost with certainty any tenderness along the course of the tendons, and diminish the ganglion if it will not get rid of it. Mr. Nunn has never met with results at all satisfactory from the employment of blister, or of localised pressure by pad and strap.

The mode of treatment which Mr. GEORGE LAWSON adopts for the small ganglia on the extensor tendons of the wrist is, first, to try if he can rupture them by firm pressure with his two thumbs whilst the hand is laid upon the table; and then, by steady rotatory rubbing, to cause the contents of the cyst to be extravasated into the adjacent cellular tissue. He then paints the part with a strong solution of iodine, and applies a firm pad and a bandage. When, however, the ganglion resists the pressure of the thumbs, and cannot in this way be ruptured, Mr. Lawson introduces a tenotomy knife through the skin, at a short distance from the ganglion, and lays it freely open subcutaneously, and then, by pressure with the fingers, evacuates its contents into the surrounding tissues. The parts are then

painted with iodine, and a pad and bandage applied, as above stated. Mr. Lawson strongly deprecates the plan of using setons for the cure of ganglia, as on two occasions he has seen the hand nearly lost from diffuse cellulitis which followed this mode of treatment.

St. George's Hospital.—Mr. HENRY LEE'S plan of treating ganglia is to puncture them subcutaneously, and to press out their contents into the subcutaneous cellular tissue every day or two until it ceases to reaccumulate. Mr. Lee lately treated in this way, with success, a ganglion as large as a French walnut, on the instep, of many years' duration.

Mr. ROUSE bursts the cyst, when recent, with his thumbs, and well rubs the surfaces together; and in many instances this succeeds. When ganglia are of long standing, he punctures them, squeezes out their contents, then rubs the surfaces, and subsequently blisters or uses liniment of iodine. If this fail, he again punctures, blisters, and dresses the sore with blue ointment. This last generally succeeds, especially if the sac be thick. In those rare cases in which melon-seed bodies exist, which are usually found on the anterior surface of the wrist, Mr. Rouse believes that no treatment save laying open the sac and dressing it in is of any avail. He has had no experience in dissecting out the sac, or laying it open and dressing in with lint, in the ordinary forms of ganglion.

London Hospital.—Mr. WALTER RIVINGTON invariably punctures the swelling with a fine-pointed bistoury, squeezes out the fluid thoroughly, and then applies a compress tightly for a few days. Failure to cure is in his experience rare.

St. Mary's Hospital.—Mr. GASCOYEN restricts the use of the term *ganglion* to a swelling which is connected with the sheath of the extensor tendons on the back of the wrist and on the dorsum of the foot. He does not regard this as an independent growth, but as a protrusion or hernia of the synovial membrane through the fibrous sheath of the tendon; and considers that its enlargement is produced by the gradual escape of the synovia into it during the movements of the tendon. Mr. Gascoyen therefore advocates a subcutaneous method of treatment, and adopts the old plan of rupturing these swellings by pressure with the thumbs or by a smart blow, afterwards applying a conical pad with a bandage. This treatment he has generally found to effect a cure in recent cases. In those of old standing, when the above means fail to disperse the tumour, he recommends free incision with a tenotomy-knife, introduced in a valvular manner through the skin; and the application of firm pressure in the same manner after evacuating the contents. When these tumours have existed for a long time, they have, however, a great tendency to re-form after any plan of treat-

ment. Blisters, iodine, and other stimulant or absorbent applications, have seldom proved of service; they merely diminish for a time the size of the swelling. The very severe inflammation which often follows the use of setons in these cases renders their employment undesirable. Mr. Gascoyen deprecates excision of these swellings, except in those few instances where the tumour, having become solid, is disposed to ulcerate from friction or some other cause. In these cases, though the connexion between the ganglion and the sheath of the tendon has become severed, so long as the contents remain fluid, showing that there is a communication with the interior of the sheath, he is strongly opposed to any attempt to remove them.

Charing Cross Hospital.—The treatment of ganglion which Mr. Hancock has found most successful, and which he now always adopts, is subcutaneous section—making, as far as possible, a crucial incision; and afterwards gradual compression, with rest of the part.

Mr. Bellamy has found that, in the case of the ordinary non-inflamed ganglia generally met with about the dorsum of the wrist or foot, a good smart squeeze with the thumbs, one on the top of the other, is sufficient to disperse the contents of the sac into the surrounding tissues. This, however, appears to be effective only in ganglia having very thin walls, and situated either above or beyond that point where the annular ligament is thickest, or exists merely as fascia. In ganglia that have existed for some time, where the walls are thick and the cyst frequently multilocular, a puncture should be made, but in a peculiar way. A sharp-pointed tenotomy-knife should be introduced flatwise through the skin, and caused to traverse the cyst or cysts, making a free opening in them. Pressure with the thumbs will then readily diffuse the contents, and a pad and bandage will frequently effect a cure. In other cases, where the thick annular ligament of the wrist is lifted up by a subjacent cyst, Mr. Bellamy prefers free incision, pushing the knife straight into the sac through the annular ligament and integument, dividing these structures thoroughly; the contents are then evacuated externally, and a strong pad or compress is kept on for some time. It is remarkable how rarely even the slightest inflammation supervenes, and how thoroughly the ganglion is destroyed without further applications. He has cured large obstinate ganglia by passing setons through them, but this was after the ordinary methods of treatment had failed.

Westminster Hospital.—Mr. Holthouse's most frequent method of proceeding in simple ganglia about the wrist is, first to flex the joint to the utmost, and make firm pressure on the tumour with the thumbs. Failing to get rid of it by this means, he passes a single thread through it; again makes pressure as

before, and so empties it through the apertures made by the thread. The two ends of the thread are then tied together, and a compress applied. In the course of one, two, three, or more days, according to the amount of inflammation set up, the thread is removed; the inflammation subsides, and the ganglion is cured. Mr. Holthouse never adopts this plan of treatment unless he can see the patient within twenty-four hours, lest inflammation of an unhealthy character or too violent should be set up. Failing this condition, he punctures the tumour with a tenotomy-knife; squeezes out the contents; applies a firm compress over it; and straps it tightly round a splint placed on the palmar aspect of the wrist. There is a variety of ganglion with the pathology of which Mr. Holthouse is not well acquainted. It appears suddenly, without obvious cause, and cannot be distinguished either by its appearance or feel from an ordinary ganglion. It differs from this, however, not only in the mode of its appearance, but in its not forming a perfectly closed sac. Under pressure, it may be made to disappear completely, and without rupture of its walls: for this reason, Mr. Holthouse always first tries pressure and manipulation of the tumour before resorting to seton or puncture.

After a considerable experience of the various methods of treating ganglion, Mr. Francis Mason believes that, in those cases in which the cyst has a thin wall, the forcible rupture with the thumb or a flat book is the simplest and best plan of effecting a cure; but in cases of long standing, or those in which the sac is very thick, by far the least painful and most successful proceeding is to puncture the cyst in a valvular manner with a grooved needle (a hare-lip pin does equally well); and, having traversed the contents, to make five, six, or more punctures in various parts of the cyst, especially on the opposite side, taking care not to wound the skin in this situation. On the needle being withdrawn, pressure with the thumb evacuates the contents of the sac into the surrounding tissues; absorption ultimately ensues; and, if the precaution be taken to place a pad of lint, secured with a bandage, over the part, the cyst seldom, if ever, re-fills. With regard to the common and popular method of suddenly rupturing ganglions with a book, Mr. Mason has remarked that, when the ganglion is tough, no little skill is required to strike the part so as effectually to accomplish the object in view; and another point of some importance is that, if the cyst be not broken at the first attempt, the patient is unwilling to have the blow repeated, much discomfort being occasioned by this somewhat apparently coarse procedure. Again, a book of suitable size and shape is not always at hand. Moreover, the proceeding is in many instances, even if successful, attended with consi-

derable pain, generally causing extreme pallor, and not unfrequently inducing a fit of fainting. Few patients object to the prick of the needle, provided they have the assurance that there is no "cutting operation." Iodine paint at the onset may be tried, but permanent good seldom follows from its use. The removal of the cyst by excision is seldom absolutely necessary; and when the operation is performed, it is, as Mr. Mason has seen, sometimes followed by intense inflammation of the sheaths of the tendons, accompanied with profuse suppuration, and leaving the use of the fingers much impaired, not to mention the constitutional irritation set up, endangering the life of the patient. Such cases are of course rare; but the practice of puncturing the cyst in the way already described is, so to speak, painless, and is almost entirely free from risk in any shape.

Mr. RICHARD DAVY pursues the following methods amongst the out-patients; viz., excision, partial or complete; subcutaneous slicing; subcutaneous puncture, and injection of tincture of iodine; mechanical crushing; and subcutaneous puncture. The seton, being a tedious and painful process, has not been employed.

Great Northern Hospital.—Mr. GAY has tried a variety of ways, with different success in different cases. His experience has led him to the conviction that these bursæ must be treated with caution, and that danger may accrue at least to the limb if such a rule be not observed in every case. Rupturing the cyst by violence, with subsequent pressure, is of little or no avail. The cyst has, in his experience, repaired the injury and become refilled. External puncture, and even free incision, followed by compression, have been more successful. A series of blisters, followed by some irritant application, such as tincture of iodine, has occasionally succeeded, especially if the cyst be subsequently punctured externally, the contents squeezed out, and compression be ultimately applied. In these cases, the blistering must be continued so as to ensure the propagation of the excited inflammatory action to the inner lining of the cyst. Mr. Gay has not tried iodine and other injections. He has removed these cysts on two occasions. and with results that determined him not to attempt this mode of treating them again. In the first instance, the operation was followed by a violent attack of diffuse subcutaneous cellulitis and acute inflammation of the wrist-joint, which proved serious, although ultimately the limb and joint were saved; and in the second case the healing of the wound was very prolonged, and was attended with deep-seated suppuration and some permanent contraction of the extensor tendon. The plan which Mr. Gay usually follows (and uniformly with suc-

cess) is that by seton. He passes a single thread of silk through the cyst, and allows it to remain until undoubted pus issues from the punctures. He employs a needle so large as to permit of the contents of the cyst being expressed through the openings which it makes; and, in case the thread does not excite sufficient inflammatory action, he not only moves it daily, but soaks the thread with tincture of iodine, and draws the portion thus soaked within the cyst. Even this plan is sometimes followed by severe inflammation and suppurative action, when the thread has had to be taken out. Occasionally, it has been so severe that he has had to open the cyst as an abscess. For the most part, however, the process is simple; no superfluous action is set up. About the seventh day, some pus exudes with the thread; the thread is then withdrawn, gentle pressure is applied, and the cyst contracts and disappears, all but its walls, Even when treated thus, the case must be under daily observation.

Mr. W. ADAMS thinks that, in the simplest form of ganglion, such as that frequently seen over the carpus, when recent, rupture of the sac by a sudden blow, or by hard pressure with the thumb, should first be tried, and will frequently succeed, even if it have to be repeated once or twice. This failing, he always resorts to a free subcutaneous section of the sac in different directions, by introducing a tenotomy-knife; and, after transfixing the sac, cutting freely in one direction, and then turning the knife, cutting as freely in the opposite direction. If the thin ganglionic sac appear to yield before the knife, the latter may be partially withdrawn, and the point made to pierce the sac in two or three situations. Firm pressure must afterwards be made by means of a piece of metal or small coin, wrapt round with lint, and kept in position by a bandage for a week. By this means he has generally succeeded in obliterating ganglia by one operation. Occasionally, however, failure occurs; and either the same operation may be repeated, or a seton may be introduced. In employing the seton, Mr. Adams always introduces six threads, and removes three on the following morning. This at once allows the fluid to escape, and relieves the inflammatory tension and pain produced by the seton. The remaining three threads may be allowed to remain for a week or more. From a neglect of this rule of removing half the seton, he once saw acute suppurative inflammation extend to the wrist-joint in a man under the care of the late Mr. Mackmurdo at St. Thomas's Hospital; and the patient died. The preparation, showing complete destruction of articular cartilages of the carpal bones, is in the museum of St. Thomas's Hospital. Mr. Adams believes the seton to be a perfectly safe and reliable remedy, if

half the silk be removed on the day following its introduction ; but it may occasionally fail, and in two instances he has cured an obstinate ganglion by a second introduction of the seton. To what extent ganglion may be dissected out with safety, he cannot say; but in one instance, mistaking the character of the tumour, Mr Adams dissected out a large ganglion of a flattened and lobulated shape, which had formed over the extensor tendons as they cross the ankle-joint. He mistook it for a fatty tumour, such as he has seen in the forearm. On section, the ganglion was seen to be thick-walled and loculated; and, from its flattened and expanded form, he does not think a seton could have been employed. Metal sutures were used, and Dr. Richardson's colloid styptic with cotton-wool applied. Union by the first intention took place. Old thick-walled bursal tumours over the patella, we know, have frequently been removed with safety.

Mr. Buxton Shillitoe always treats simple ganglion by subcutaneous puncture with a grooved needle, thoroughly emptying the cyst, and irritating its interior by a few scratches with the point of the needle. He is particular in keeping pressure upon the emptied cyst until he can apply a firm pad, which is retained in place by plaster and bandage for a week or so. It very seldom fails to cure, if thoroughly done. If it do, he repeats it immediately the cyst commences to refill. After the bandages are removed, he orders the application of iodine or frequent douches of cold water. Mr. Shillitoe has seen awkward results from the use of setons; and he believes, in the treatment of simple ganglions, that they are unnecessary. Compound ganglion requires much more careful treatment, partly on account of its extent and position, partly because its contents are mixed up with more or less solid fibrinous deposits. They occur chiefly in the palm of the hand and side of the sole of the foot. He has cured those on the palm of the hand by dissecting out a portion of the cyst, without division of the annular ligament—squeezing out the remainder of the semi-solid contents, and allowing the wound slowly to granulate up. Mr. Shillitoe says he should not hesitate to inject iodine in such cases.

University College Hospital.—Ganglion at the wrist Mr. Berkeley Hill never forcibly ruptures, but evacuates subcutaneously by first incising the sac freely with a tenotome, then squeezing out the contents of the sac, and firmly pressing the walls together with a compress and strap of plaster. Should it recur, a fine silk thread is carried through the swelling with an ordinary suture-needle, and left for three or four days. To prevent the ligature from exciting too great irritation and sup-

puration, the patient is always directed to attend the following day at the hospital, that a dresser may examine the seton, and remove it if the action be too great. Injection of iodine is a less satisfactory mode; but it is employed by Mr. Hill for the deeply-seated ganglia of the ham, where the abscess that may follow the seton is extremely troublesome to heal. A ganglion now and then forms between the tibia and the inner hamstring, which Mr. Hill has found very tedious. In one case, the patient was obliged to relinquish his occupation through the pain which he felt in walking. His ganglion, being close to the knee-joint, was at first simply tapped and injected with iodine; but, as the fluid accumulated again, a thread was passed through, which was worn a week without much inconvenience; and the swelling gradually grew less, but did not disappear wholly, causing, not in ordinary walking, but when the limb was used to raise the body, as in going upstairs, a tolerably sharp pain. An attempt was then made to dislodge the hard nodule subcutaneously; this was so far accomplished that the tumour vanished, but some pain is still felt on going upstairs. The knee-joint was quite unaffected throughout these experiments. The ganglia that result from fluid collecting in the synovial sheath common to the tendons in the palm, and in that which lubricates the peronei behind the outer malleolus, Mr. Hill has tapped and injected with success, and without causing troublesome symptoms or stiffness afterwards. The application of pressure and blisters over these compound ganglia, in Mr. Hill's experience, is a waste of time, as these agents do not cause absorption of the fluid.

Great Northern Hospital.—Mr. VINCENT has treated cases of ganglion in various ways, and with variable success. Forcible rupture by digital pressure, followed by continued pressure, and, if possible, a splint, have given the best results. Mr. Vincent considers the use of the latter advisable, to obtain the perfect cure of ganglion, as, in some cases of old standing, the ganglion, instead of being confined, as in recent cases, to the tissue between the tendon and the skin, extends around the tendon, and thus every movement of the tendon after the rupture prevents the adhesion of the walls of the sac; and hence the recurrence of the disease, and the necessity for the splint in cases where the patient can and will submit to its application. In private practice, the splint should always be insisted on. As hospital patients, however, refuse to discontinue their work, this plan is frequently impracticable.—*British Medical Journal, July* 1, 8, *and* 15, 1871, *pp.* 9, 36, 64.

59.—AN INSTRUMENT TO FACILITATE SKIN GRAFTING.

An instrument designed to facilitate the process of skin-grafting, was brought into use at St. Bartholomew's Hospital in the early part of last March. It consists of a pair of curved scissors, which are provided on their concave surface with bent forceps. These are controlled by a lever which descends with the separation of the blades, and rises when they are brought together. The movements of these several parts are so concerted that the forceps meet between, and just below, the blades, immediately before the closure of the latter, and then rise between them to such a height that whatever they have seized will be divided from its attachment when the blades actually meet. Thus the whole process of seizing a small portion of skin, separating, and raising it, can be almost simultaneously performed with one hand. The size of the severed piece of skin is proportionate to the force with which the forceps are pressed against the surface from which it is to be removed.

This contrivance was suggested to Mr. Ferguson, of Giltspur-street, early in the present year, by Mr. Crips, a student of St. Batholomew's. It has since been in constant use at the hospital, and is very favourably spoken of. Mr. Pollock also has, we believe, expressed his approval of it.—*Lancet, June* 3, 1871, *p.* 745.

60.—ON OAKUM AS A DRESSING FOR BURNS.

By Dr. Herbert L. Snow, Shrewsbury.

I wish to bring under the notice of readers of the journal the value of common picked oakum as a dressing in the suppurating sores resulting from burns. Among the good effects which I claim for it are these: First, it ordinarily induces the cicatrisation of extensive sores with remarkable rapidity; secondly, it induces healing action in such of these as, occurring in debilitated subjects, or under defective hygienic conditions, have almost assumed the character of indolent ulcers, and remain stationary under the application of the ordinary lotions; thirdly, I may allude almost to the entire absence of any offensive smell under the use of oakum, to its cheapness, and to the saving of time and trouble effected by its employment; lastly, (and this I hold to be by far the most important point of all) the *resulting cicatrices do not contract*. I have not yet used oakum in a sufficient number of burn-cases to advance the last statement as absolute; but in the limited number in which I have been able to employ this dressing thoroughly (in several

of these there were very severe and extensive sores), I have always found the cicatrix of a peculiar dark-reddish colour, and soft consistence; none of the usual tense fibrillated appearance has been visible, and no subsequent contraction has ensued.

I now always use the common oakum, which appears to me to answer much better than the recently introduced "tenax," and probably contains more of the tarry principles. This is wetted with cold water, then placed on the surface of the sore, and a bandage loosely applied over it. No pain follows the application; but the oakum has to be wetted (without being removed) with cold water several times a day, or the patient will complain of its feeling dry and sticky: the latter may generally be left to do this *ad libitum*. The dressing need not be changed oftener than on alternate days—sometimes not until every fourth or fifth day, according to the condition of the wound and the amount of suppuration present.

Under this application some very large and deep sores heal with marvellous rapidity—many which would not be well for months under the ordinary modes of treatment, cicatrising in the course of as many weeks. All make very favourable progress towards healing, but in some the process is much more rapid than in others. The granulations seem to have a much diminished tendency to become exuberant, and seldom require either pressure or the application of escharotics. The oakum should not be applied until suppuration is well established; previously, it does not seem so beneficial as many of the applications in ordinary use. It may also be used with benefit for suppurating ulcers resulting from other causes than burns; but I have not found the former heal with nearly so much rapidity as the latter.

The last of the considerations which I have adduced above must be my main apology for sending this paper, which I have done in the hope of inducing further trials of the material. Admitting that my own experience has not yet been sufficiently extensive to decide the point absolutely, if indeed we acquire a means of preventing the frightful sequelæ of burns, unfortunately so common, we shall have made a material contribution to the alleviation of human suffering.

I may mention, in conclusion, that I have lately adopted Mr. Skey's plan of applying a solution of nitrate of silver (twenty grains to the ounce) to all recent burns, with the best effect; the pain being soon much alleviated, and the subsequent ulceration considerably diminished.—*British Medical Journal, June* 10, 1871, *p.* 610.

SYPHILITIC AFFECTIONS.

61.—SYPHILIS AND VACCINATION.

[The following remarks on this subject are from the *Medical Times and Gazette* and the *Lancet.*]

The Committee appointed by the Royal Medical and Chirurgical Society upon the cases of vaccino-syphilis referred to in Mr. Hutchinson's papers, made their report on Tuesday evening. They state that they saw three of the cases in each series, and also the two vaccinifers. We are sorry to say that the Committee confirm the inferences of Mr. Hutchinson. They have seen no reason to doubt the fact that syphilis was in each series conveyed in vaccination, but whether through the medium of the lymph, or of blood, or both, they are not prepared to state. At the same meeting it was announced that Mr. Hutchinson had forwarded an appendix to his former papers, but it was not read in consequence of the meeting being the last for the session. We are far from wishing to throw undeserved obloquy upon the practice of arm-to-arm vaccination; but it cannot reasonbly be expected that the public, who are always more open to unfavourable than to favourable reports, will ignore the facts now established. There is a danger—infinitesimal indeed, but still a danger—and they will demand to be protected from it. No one who knows what syphilis is, and how it impresses its stamp upon a whole lifetime, will say that such a demand is unreasonable. At the present moment no one can tell how often a similar accident has occurred before; for these cases can scarcely be believed to be unique. And the question which now presents itself to us is, what steps the Government intends to take to provide that the population shall not plead danger as a bar to the vaccination of their children. Much has been written about the non-success of heifer vaccination, and much which is of questionable authenticity; but, after all, it is due to the public that the alternative should be offered of vaccination from the child's arm or of vaccination from the calf. In London, Manchester, Liverpool, and other large towns, we certainly think that, at definite and short intervals, an animal vaccinifer should be provided by the State for the use of persons who object to arm-to-arm vaccination. A few calves thus provided at large centres of population would suffice for the exigencies of the case, since it has been shown that the lymph can be preserved for several days upon thoroughly charged points, and, if then used by the method of scarification, is nearly as efficient as humanised lymph.—*Medical Times and Gazette, July* 1, 1871, *p.* 13.

[The Editor of the *Lancet* says :]

It is especially necessary that we should call attention to the Report of the Medical and Chirurgical Society's Committee on Mr. Hutchinson's cases of alleged transmission of syphilis by the medium of vaccine lymph, because a most extraordinary misrepresentation of the purport of that document has been published in the columns of a contemporary. The journal has stated, no doubt by inadvertence, that the Committee have confirmed the diagnosis of syphilis in Mr. Hutchinson's cases. This is a most incorrect verson of the report. It must be in the recollection of everyone that two series of cases were brought forward. In the first of these a number of patients, of different families and of various ages, vaccinated from the same child, were affected at the site of the operation with sores which presented a considerable resemblance to indurated chancres; in the other series, two children born of the same parents were affected after vaccination from another vaccinifer, not merely with local symptoms resembling chancre, but with symptoms strongly resembling those of true secondary syphilis. In the first group of cases (Mr. Hutchinson's own) there had been full opportunity for verifying the fact that the vaccinifer was syphilitic, and that the vaccinations had been performed with all proper care, save in one respect —that there was reason to think that blood as well as lymph had been taken from the vaccinifer. In the case of the other two children (patients of Mr. Warren Tay) there was, on the contrary, no sufficient evidence forthcoming to determine the origin of the syphilitic infection.

Such, in a few words, was the position in which the matter rested after the discussion at the Medical and Chirurgical Society on May 16th, at which time the subject was handed over to a Committee for further investigation. In our leading article of May 20th we pointed out the essential dfference between the two series of facts so far adduced ; we showed that in Mr. Hutchinson's cases there was unmistakable evidence that the vaccinifer had communicated one and the same *irritant* influence to a variety of persons whose constitutions had nothing in common with each other, but that until undoubted secondary symptoms should appear in the sufferers there was no reason for assuming that that irritation was due to the communication of syphilitic virus; while in the two cases of Mr. Tay there was, unfortunately, very strong evidence of syphilis, but so far no sufficient proof that the disease had been communicated in vaccination. Let us add that Mr. Hutchinson himself believed the patients of his first series to have so far escaped from secondary symptoms because they had been treated with mercury.

Now the further evidence produced by the Committee is this:—As to Mr. Hutchinson's cases they report that the whole thirteen still (five months after vaccination) remain free from secondary symptoms; and consequently there is, at present, *no proof of syphilis in these at all.* As to the cases of Mr. Warren Tay, unhappily, the result of inquiry is very different. In addition to the brother and sister originally known to be syphilised, two other children vaccinated from the same vaccinifer, and not related to each other, have been found to have also exhibited signs of constitutional syphilis. It must be confessed that, so far as this fact goes, it considerably strengthens the probability that vaccination was the medium of the infection. But, on the other hand, there are enormous gaps in the chain of proof. It is not merely that the Committee had no personal interview with the vaccinator, though that is a serious defect. What is of far more consequence is that the evidence as to the vaccinifer of Mr. Tay's cases having had syphilis is about the slightest on which we ever heard of a diagnosis being founded. On that subject we may refer our readers to what we said on a former occasion (*Lancet*, May 20th); and we may now add that the Committee have not obtained one morsel of additional evidence that the vaccinifer was syphilitic, except the fact that two other children among those vaccinated from it became syphilitic. Our readers do not need to be told, however, that it would by no means be anything very extraordinary if four children, belonging to three families, who all happened to be vaccinated from the same child, should all have suffered from syphilis of separate origin. In short, Mr. Tay's cases, though warranting a certain amount of suspicion, are far indeed from affording proof of vaccino-syphilis. As to Mr. Hutchinson's cases, we are abundantly justified in saying that, so far, their value as proofs is absolutely *nil.*

Upon the whole we consider that very grave errors have been committed by nearly everyone concerned in bringing these cases forward. The inquiry in its crude early stages was one that was not fit to be undertaken by a miscellaneous body of medical men like the Fellows of the Medical and Chirurgical Society; very few of whom could be supposed capable of deciding, in the brief period of a debate, the precise value of evidence on an intricate question of pathology, or of recalling (even if they had ever studied) the whole previous history of the vaccino-syphilis controversy. We think there can be no doubt that the whole preliminary investigation should have been conducted by the Medical Officer of the Privy Council and his able subordinates; and that the evidence should only have been presented to the general profession for discussion when it had undergone a most rigorous sifting at their hands. We repeat what we said before

—that it is a descreditable exhibition when prominent members of a great Society like the Medico-Chirurgical actually declare that So-and-so is syphilised on the simple evidence of his having an indurated sore on the arm which had reopened after healing up. Why, that is a common characteristic of the effects of unusually irritant lymph of any kind; it is a phenomenon which has occurred to many practitioners during the recent small-pox epidemic; and it is a specially frequent and unwelcome result of that same clumsy and discredited heifer-vaccination which our sage contemporary, already referred to, seeks to bring into fashion again! Readers of the *Lancet* have only very recently had an example of the latter fact brought before them by Mr. Trotter, assistant-surgeon to the Coldstream Guards; a series of patients having had sores exactly resembling chancres after vaccination from the cow.

We must conclude these remarks by earnestly reminding those of the public who may chance to read this article, that even supposing the worst were proved which it was sought to prove regarding these cases, it is only a microscopic danger that has been shown to exist, and one that the reasonable care which a respectable practitioner may be expected to exercise will altogether set aside. There is not the shadow of proof that vaccine lymph itself ever infects; and we hope we may confidently state that after the recent discussion practitioners will be doubly careful to observe that old and well-founded rule—never on any account to employ lymph which contains even the smallest speck of blood or any foreign matter. And undoubtedly they will also still more strenuously carry out the injunction which all instructed men knew to be binding long before the recent controversies were heard of—by all possible means to avoid taking lymph from a child that is unhealthy either in itself or in its family history.—*Lancet, July* 8, 1871, *p.* 52.

62.—ON SYPHILITIC INOCULATION WITH REFERENCE TO THE PRODUCTION OF THE SPECIFIC PUSTULE.

By John Morgan, Esq., Professor of Surgical and Descriptive Anatomy, R.C.S.I., &c.

Mr. H. Lee in some remarks on this subject, quoted from his lectures in the *Medical Press*, April 19th, has alluded to observations which have been made and published by me in Dublin, as to the production of certain chancroids or sores, simple in character, from the secretions or discharge of contaminated patients on patients also contaminated.

The question is one of interest—as the observations and direct investigations I have made tend to show that the chancroid or

soft sore is but a descendant from the chancre or hard sore, and is not a distinct poison, but can be produced from the discharge of a constitutionally infected person, and more especially from one form which is an obvious and fertile source—the vaginal secretion of an infected female.

Mr. Lee says, alluding to my testings :—

"It will be said it is the soft sore which has been produced in the experiments alluded to. Now, to explain this, two circumstances must be borne in mind : First, in certain states of constitution in syphilitic subjects, a pustule and subsequent sore is produced with great facility. Here is the drawing of a well-developed pustule in a syphilitic subject, produced by the inoculation of some pus from a case of excision of the knee-joint in a child. The second point to which I advert, and which has a more direct bearing upon the experiments in question, is that inoculations may no doubt be performed occasionally on syphilitic patients which will not present the characters of either of the forms of disease which I have described. I have inoculated and succeeded in inoculating syphilitic patients from the secretions of indurated sores, but the ulcers produced have not presented the sharp ulcerated border of the local suppurating sore, nor have they been accompanied by the adhesive form of inflammation of true syphilis."

As to the first point—It may readily be admitted that the inoculation of pus produced a pustule in a syphilitic patient, but —Was it a specific pustule? Was it the same as a pustule the result of specific inoculation? Did it differ for instance from the pustule of a poisoned wound? Was it *re-inoculable* on the patient's self or on others? If it was not *re*-inoculable, and capable of becoming the head of a series, we must eliminate the occurrence of such an inoculation as being valueless in the argument. Inoculation with pus from an excised knee-joint would be likely to produce a pustule on any one, specially if not in vigorous health. I saw not very long since a well marked pustule produced on the back of the hand of a surgeon in an hospital by simply the contact of a drop of blood, yet I would be extremely sorry to brand him as syphilitic on that account, and I am satisfied the case was not syphilitic. The remark is made that in certain states of the constitution in syphilitic subjects a pustule and subsequent sore is produced with great facility; and it is mentioned by Mr. Lee "that it has been proved over and over again that the matter of gonorrhœa is not inoculable." I have tried it on several occasions on syphilitic subjects, and when taken from males who never had a sore, yet it failed. If a pustule and sore can be produced by inoculating with matter from an excised knee-joint, why should the gonorrhœal matter fail to do so? Is it not presumable that the inoculation of the

matter from a diseased joint acted on a syphilitic patient just as it would on any other person by producing a pustule, which was not specific or specifically inoculable, though it may have been highly irritable.

It appears proved that inoculation from the gonorrhœal discharge of a *male* produces no result on a healthy person, or what is more important in the question debated, *on a syphilitic* patient, yet that the gonorrhœa or vaginal discharge of a syphilitic *woman* will produce a *re*-inoculable pustule on a syphilitic patient, and what is also to be specially borne in mind, on the patient's *own* person—illustrating the well-known argument of those who hold to the existence of *two* poisons—that the produced sore is of the *auto*-inoculable character of the chancroid or simple affection.

The case of Hunter is referred to by Mr. Lee, as an evidence of the power of the gonorrhœa "of a syphilitic subject," to give, as it did Mr. Hunter, an indurated sore; this I do not consider a very unreasonable suggestion, and that matter was derived from a syphilitic male who may have had a sore in existence, or been syphilitic. The gonorrhœal or vaginal secretion of a syphilitic female, constitutionally and not locally affected, however, I have found to produce the soft simple sore, with its characteristic pustule, when inoculated on syphilitic subjects (as they alone were suited for such a test). To this it has been objected by Mr. Lee, that "the experiments have all been performed on patients whose systems were already affected by syphilis," and that the same difficulty would attend as if experimenting "with cow-pox on those who already had the disease." But, unfortunately, the theory of the dualists is carried out by the virus got from this source, it is *auto*-inoculable. The gonorrhœa of a syphilitic male should have been inert on Hunter a second time, or on anyone "already affected with syphilis," but the inoculation of the vaginal discharge of a syphilitic woman is *auto*-inoculable, and is preceded by the characteristic and highly re-inoculable pustule which attends the inoculation of a soft sore produced by direct inoculation from another soft sore—or from a sore the product originally of artificial inoculation with a syphilitic vaginal discharge. The appearances and conduct of both sores *are identical*—the one a descendant in a direct line from a vaginal discharge, the other the descendant of a chancroid or soft sore.

The following testings which I carefully performed prove abundantly the *auto*-inoculability of this discharge, and its general inoculability on other syphilitic patients, and proves that the ordinary sore or chancroid, with its characteristics of inoculability, and auto-inoculability, and pustular development, may be the direct product of contamination by a secondary discharge.

July 6, 1870.—A strong, fresh-looking girl of twenty-three, had been admitted to hospital on two occasions:—First, on June 1, 1868—for several soft sores; secondly, on October, 1868—for a sloughing phagedenic sore of the nympha, followed by patches, papules, pains, and finally by ecthymatous spots here and there over the body: the phagedenic ulceration was most acute, and was cured by the use of escharotic and local applications.

She remained without further symptoms till about ten days before admission, when she got a discharge with some pain and tenderness, and a sore formed of a secondary character, at the cicatrix of the former sore near the nates; the stains left by the ecthyma are still very evident; also a wound on the arm, made about six weeks ago, presents a deep coppery stain, indicative of a latent taint.

On careful examination with the speculum, no ulcer whatever is to be seen, but there is a purulent vaginal discharge.

I inoculated with this on herself, on July 7, producing, by the 11th, a well-marked pustule, becoming a sore shortly afterwards.

July 7, I inoculated No. 1193, suffering from copious patches, alopecia, and pains, and produced, by the 11th, a characteristic pustule and sore.

On July 7, I inoculated No. 921 (who was at this time partially under the influence of another inoculation), and produced, by the 11th, a perfectly formed umbilicated pustule. This patient was intensely syphilitic at the time.

On July 7, I also inoculated No. 988, who was almost free from syphilitic symptoms, having been frequently inoculated, but produced only a small ill-formed pustule and superficial sore.

July 7, I also inoculated No. 1000, who had already been several times inoculated, and produced a specific, but not vigorous pustule.

July 7, I inoculated No. 1075, and at the time suffering from a vigorous chancroid; and produced a perfect pustule and small chancroid.

This case is very interesting, as there was no rash, and the secretion from the sore could not *possibly* be mingled with the vaginal discharge. The sore had not the character of a primary either in appearance or secretion, and the patient might otherwise have been looked on as a specimen of rude health, but as evidenced by the stains, and the discoloration left by the wound occurring within two months, together with the re-opening of the original cicatrix, the venereal taint was, though latent, still as active as ever.

It is suggested that such a testing as this is but "a different

form of the experiment Hunter performed on himself," but there is no evidence that the source of Hunter's virus was a constitutional secretion. I confess I think it is widely different as to result and form.

1. No indurated sore was produced.
2. The secretion or gonorrhœa of constitutionally infected *women* was used.
3. These were undoubtedly syphilitic patients.
4. The recipients were all undoubtedly syphilitic.
5. Inoculations and also *auto*-inoculations were performed.
6. The resulting sores were re-inoculated in many cases.

I have arranged in a tabular form a few of these cases. The result narrows the question to this: either these inoculations would have produced chancroids or soft sores on the individual herself, or on an infected male; and be capable, at the same time, of producing (as Mr. Lee remarks in Hunter's case) an indurated sore on an *un*tainted person; or else this is the true origin of the chancroid or soft sore, which is but a descent, with modification, from syphilis proper, and derivable from a secondary lesion; that it would produce "indurated sores" cannot be reconciled with the every day fact that indurated sores are comparatively unfrequent as compared with the simple sore which, as in the accompanying series, is proved to be most easily and vigorously reproducible from a discharge only.

Remarkable Inoculations from Vaginal Discharge—all producing Characteristic Pustules and Chancroids more or less persistent, capable of Indefinite Propagation.

Inoculation of the Vaginal Discharge of No. 1204.

Produced chancroid on No. 1118, on June 2nd, which produced chancroids	June 10, on No. 1000.
	Do. 12, on No. 1000.
	Do, 12, on No. 1085.
	Do. 12, on No. 1159.

Inoculation of the Vaginal Discharge of No. 988.

May 23,	do.	on herself.	From this, June 3, on herself.
			Do. ,, 10, on 1163.
			Do. ,, 12, on 1112.
June 3,	do.	on herself.	
,, 13,	do.	on herself.	
,, 20,	do.	on 1060.	
,, 23,	do.	on 921; not very developed.	
,, 23,	do.	on herself.	

Inoculation of the Vaginal Discharge of No. 1185.

June 27,	do.	on herself.	From this, June 27, on 1167.
,, 27,	do.	on 921.	
,, 27,	do.	on 911.	
,, 29,	do.	on 1169.	
,, 30,	do.	on 1189.	

Inoculation of the Vaginal Discharge of No. 1140.

June 7,	do.	on herself.
,, 7,	do.	on 1112.
,, 15,	do.	on 1138.

Inoculation of the Vaginal Discharge of No. 1220.

July 6,	do.	on herself.
,, 7,	do.	on 1193.
,, 7,	do.	on 921.
,, 7,	do.	on 1000; not very vigorous.
,, 7,	do.	on 988; ,, ,,
,, 7,	do.	on 2075; ,, ,,

No. 1118 proves the power of this source to become the head of a series, or a chancroid, if not a truly syphilitic "scolex."

No. 988 proves the auto-inoculability of the auto-inoculated sore itself.

The remainder show the *auto*-inoculability and *re*-inoculability of this contagion source.

Mr. Lee remarks, as before quoted, that "in certain states of the constitution in syphilitic subjects a pustule and sore is produced with great facility." Now, in the above-noted cases, twenty-eight of them, taken throughout the wards and in various stages, some having been before inoculated, and others not, the pustule and sore were always similar, and, in some series (for instance, No. 1220), the results were not as active perhaps, but were as marked as in No. 1185. "The certain state" I found a uniform one, and the only cases where there were inferior developments were those that had been *previously* inoculated. This is a very remarkable fact, and it would be an interesting addition to our knowledge if we were acquainted with, or could prove, "the certain state" where this pustule is easily produced, if, indeed, such a state absolutely exists; it would go far in explaining the phenomena of soft sores, or the possibility of specific pustular sores occurring in syphilitic patients from contamination or accidental inoculation with *any* form of genital pus (as with the pus from the knee-joint above referred to); but this occurrence I must look on simply as an accident, until it is proved that the resulting sore was specifically re-inoculable and auto-inoculable.

Allusion is made to the co-existence of the two forms of sores, and that the super-addition of the virus of the simple sore may overwhelm the appearances of the sore characterized by "adhesive action" around it; and when artificial inoculation is performed from this, the "characteristic simple sore with the sharp edge was produced." Mr. Lee says that it is the secretion of the superadded simple sore virus, which is alone transmittable; but he mentions elsewhere that when he had irritated the sore characterized by adhesive action (or the hard sore) by savine, &c., and thus procured *pus*, he then could inoculate successfully. It is surely reasonable to conclude that the pus-producing virus will have acted just as the savine did, and gave the fertilising secretion, the cells of which seem, by Rollet's experiment (of filtering the pus, and then finding it inert), to carry the morbific element perhaps in the shape of "microzoons," such as have been described in the vaccine fluid. The remarks of Sperino, where a lancet, which had been charged with chancroid pus and left to dry for seven months, and thus had thoroughly disturbed the constitution of the pus-cell, and yet produced inoculation, proves that the morbific atoms do not consist of the perfect pus-cell, although filtration of them checks contagion—they must exist independently of the pus-cell, but have some relation to them not as yet determined. The mixed sore is a pleasant half-way house on the road to the syphilitic Parnassus, but that the virus of induration and adhesive inflammation and of ulceration and suppurative action should co-exist seems contrary to our practical observation.

With regard to this question, it it said that, "supposing it to have been an indurated sore, upon which another secretion was inoculated, it would be the latter only which would be communicated by inoculation," though one sore is characteristically auto-inoculable, and the other, as *soon as it furnishes* pus, is also admitted to be so, it is said that it was the superadded secretion which is alone inoculable. Under such circumstances, it is, I think, very problematical as to which poison would furnish the inoculative power, and there can be little doubt as to the infecting operation of such a sore, if communicated. What its aspect would have been, or under what form re-inoculations or auto-inoculations appeared, if tested, is not shown, but would be interesting. The cases of inoculation on healthy subjects occurring in the thumb and also on the breast, and illustrated lately in this journal, were undoubtedly from congenital patches, yet the primitive lesion was of the nature of the chancroid or soft pus-secreting sore, and yet was followed by secondary affections. The vast majority of sores occurring in *female* patients in the Lock Hospital here, are pus-

secreting and chancroid in appearance, yet are almost invariably followed by constitutional signs of various forms. I have auto-inoculated such sores frequently, I have opened the suppurating bubos accompanying such sores, and I have auto- and re-inoculated from the vaginal discharge of such patients, and produced true inoculative pustules and chancroid inoculable sores, the patients themselves offering, at the same time, intense syphilitic phenomena; this is, no doubt, not conformable with the dual theory, but it is a substantial fact, and the records of the hospital amply corroborate the statement.

Mr. Lee lays down as a rule that, "if from any depressing influence, &c., the primary affection has ulcerated to any extent, or suppurated for any length of time, then the secondary affection will have a tendency to do the same." The cases above alluded to suppurated freely, but the great majority of them had no ulcerative secondaries, and every day we see papules and pustular rashes co-exist, and one exchanging with the other, as it appears to me, quite irrespective of the appearance of the primary. I hope to recur to the matter at a future period, I wish at present to draw attention to it as a startling enunciation.

I have no doubt but that the great source of the chancroid or soft sore in men is from secondary syphilitic secretions, the viginal discharge being obviously the most copious, and the most obnoxious, is capable of auto-inoculation and of re-inoculation in an indefinite and intensifying series, and though the sore may, and very frequently does, exhaust itself in mere local phenomena, it may, at the same time, carry with it sufficient of its parent virus to contaminate constitutionally. It is impossible that females suffering from soft sores, which speedily become intensely painful and intolerable, could permit coitus, whereas how large a percentage of the male population suffer from soft sores is too well known to practitioners; some other source must exist in the female than the direct effect of a local or soft sore communicating by contact.

At the present moment I have in hospital a patient whose entire penis has been carried away by ulceration, with the only sore he ever had, and he is now thoroughly broken down with constitutional syphilis; a young man of twenty-one, covered with stains, the result of a sore and suppurating bubo of two years ago; a boy of sixteen with sore and suppurating bubo still unhealed, having secondary rash and pains; a boy of seventeen, with a large, soft sore, equalling a crown piece, yet with well-marked indurating glands. On the other hand, very many cases will escape the tendency of recurrence to the original source, however constant; just as we see in nature, from

the most luxuriant apple grafted on the "crab," the seeds will produce "crabs," but the graft will produce grafts of equal, if not improved quality; nevertheless, the tendency of recurrence to the parent source will persistently show itself in the seed if sown, and the "crab" will again develop itself.—*Medical Press and Circular, May* 3, 1871, *p.* 369.

63.—SYPHILITIC ERUPTION RESEMBLING RUPIA.

[The following is from a report of Mr. Hutchinson's Clinique at the London Hospital.]

A curious case of syphilitic disease of the brain was called into service to demonstrate a syphilitic eruption on the body. This eruption was very abundant, serpiginous, and multiple, and hence resembling rupia. It was stated to be not rupia, however, because (1) it was a tertiary, and not a secondary symptom; (2) the ulcer healed in its centre, whilst it spread at its edges; and (3) it was not symmetrical. The patient was taking twenty-five grains of iodide of potassium daily, and Mr. Hutchinson pointed out the necessity of commencing with small doses of this drug, but steadily pushing it on to get the best effect, there seeming really no limit in some cases to the advantageous pushing.—*Medical Times and Gazette, July* 1, 1871, *p.* 8.

64.—ON THE MODERN TREATMENT OF SYPHILIS.

By Edgecombe Venning, Esq.

[In the year 1864 the Lords Commissioners of the Admiralty appointed a Committee to enquire into the pathology and treatment of syphilis. This Committee published its report in 1867, and it is upon this report that the following paper is chiefly founded. Fifty-six of the most eminent members of the Profession were examined before this committee, and their evidence demands close attention. The writer gives us a concise statement of the conclusions to which the evidence brings us upon this interesting subject. He observes that we may best ascertain what conclusions may be drawn from this evidence by enquiring:]

1. Are there two forms of venereal sores, depending upon different poisons, and whose effects on the constitution are essentially different?
2. Can one form of sore always be distinguished from the other?

On the first question forty witnesses gave evidence. Of this number, thirty-five were of opinion that there are two different

forms of sores; and only five took a contrary view. And I believe that most surgeons in the present day will at least admit this much; as it must daily fall to the lot of most of us—who have a fair amount of experience in such cases—to see the two forms well and clearly defined: one represented as a soft sore, secreting pus plentifully; and the other characterised by an indurated base, and not as a rule discharging pus, but only the *débris* of lymph. But when the fact of the two sores depending upon separate poisons is investigated, we do not find such unanimity of opinion. Sixteen witnesses only coincided with this theory; twenty-two were decidedly against it; and two were doubtful about it. It is for us to inquire into the causes of this disagreement, and try if possible to clear up the existing doubt. In order to do this as effectually as possible, I propose to examine into the *pros* and *cons* that were put forward in evidence.

Those who stated decidedly that there were two different poisons, always producing different sores and sequelæ, were very few in number; but there were some, and among them was Dr. Byrne, surgeon to the Westmoreland Lock Hospital, Dublin, who stated that "the soft sore is never followed by constitutional symptoms, unless there be some modification of it." The majority of the witnesses who were in favour of the double-poison theory stated, in addition, that they had not infrequently seen the soft variety of sore followed by constitutional symptoms. I account for this discrepancy in the following manner. The soft sore which has been followed by secondary symptoms has been a soft sore with something added to it; and that something has been the true syphilitic poison; in fact, the local and constitutional disease have existed together. I believe that in the majority of these instances has there been a certain amount of induration accompanying these sores, though it may have been so slight as to escape anything but most minute inspection. But I admit that in some instances the induration upon which so much stress is laid is altogether absent. I cannot help thinking that too much importance has been attached to this one symptom. That when present it is a very valuable one, I frankly admit; but its absence does not negative the fact of the sore being a true infecting one. Mr. Henry Lee, in his published lectures on *Practical Pathology*, vol. ii .p. 8, makes some very sensible remarks on this point. "A physician," he says, "may be unable at once, or at all, to form a diagnosis of a particular kind of fever or disease from the presence or absence of one symptom; but he can, and does daily, form a correct diagnosis by the consideration of a group of symptoms, and the conformity or nonconformity of his patient's illness to it." The infecting chancre is no exception

to this rule. We must be guided by a series of symptoms; and I hope to be able to show that it is quite possible by carrying out this rule, to diagnose each form of sore with certainty.

I am led to the conclusion, that it is the mixing of the two poisons in one sore which has led to so much disagreement, by personal observation of a large number of cases. And I cannot call to mind any case of soft chancre, which was purely such (and by this latter I mean unattended by any of the signs of an infecting sore,) that was followed by constitutional symptoms.

Twenty-two witnesses, I have stated, were decidedly against the theory of the duality of the poison; and we must inquire into their reasons for believing thus. Here, again, we shall see that their great reason was, that they had so frequently seen the soft sore followed by constitutional symptoms. Our answer to this must be the same as before—viz., that they have been dealing with mixed poisons. But supposing, as is the case with many, that they will not admit of this solution of the question. How, then, do they account for such very different results from one poison? Some would lead us to believe that the soft sore is only a modification of the genuine syphilitic poison. By some means it has been diluted, and therefore its effects have become less potent. Others state that the matter with which the system has become inoculated has been old, and thus become degenerated; and a third lay all the stress upon the constitution into which the virus has been received. "As your soil, so will your fruit be," say they. I cannot agree with any one of these theories, nor do I think that recent investigations tend to confirm them. That a diluted poison should produce such very different results, is hardly conceivable. Surely the constitution would be tainted to a greater or less extent, and would give some greater evidence that so noxious an element had been received into the system, even though diluted, than a small sore, which heals in a given time, and gives no farther trouble. With regard to the degeneration of the poison; if it has become so degenerate, it can no longer be looked upon as the same thing, and must be classed among other decaying and effete matters. Nor do I think that the third theory is more tenable. That the condition of the constitution shall determine whether such a virulent poison as syphilis is to produce only a local or general contamination, is, I think, unreasonable. We do not find this to be the case with other poisons. If the poison of variola is inoculated into a system which has not previously been infected with it, we see definite results, and it runs a known course, whatever the state of the constitution; and so with other diseases depending upon a specific poison. We do not see them at one time producing a local result, and at another a general one. Why,

then, is syphilis to be a great exception to this rule? I do not believe that such is really the case. In other systems, "as your soil, so will your fruit be," does not hold good. If wheat be planted in a field, it will not depend upon the soil whether the fruit it yields will be wheat or oats; into whatever soil it is placed, the result is always the same; and such is the case with the human soil.

With regard to the treatment. Here, again, we are met with scarcely less diversity of opinion than we have found to be the case with regard to the pathology. We have to contend with mercurialists and non-mercurialists, including under the latter title those who advocate syphilisation as a means of cure, and those who recommend that the disease should be allowed to run its natural course without interference. In order to ascertain what conclusions may be drawn from the evidence given on these several points, we will consider the following questions:

1. Should mercury be administered in all forms of primary sores?

2. Is mercury beneficial in any particular form of primary sore? If so, in what form should it be administered?

3. Is mercury beneficial in the secondary evidences of the disease, or is there another therapeutic agent which gives better results either alone or in combination with mercury?

4. Are the results of the non-mercurial form of treatment as successful as the mercurial?

On the first of these questions forty witnesses gave evidence. Of this number, ten were of opinion that mercury should be administered in all forms of primary venereal sores. Most of them recognised but one form of sore, and treated it accordingly. On the other hand, eighteen stated that it was not necessary to give mercury in all primary sores. These recognised the two distinct chancres; one (the soft) constituting the local, and the other the infecting or constitutional disease. Four witnesses stated that they never gave mercury in any form of primary sore, and three were doubtful whether they should not give it in both kinds. Mr J. R. Lane stated, "that though he did not always treat the soft form of sore with mercury, still he believed that it would be always safer to do so." Five witnesses never gave mercury in any form of the disease from beginning to end; three of the latter number treating all their cases by syphilisation.

From what has already been stated with regard to the pathology of the disease, it will be surmised that I hold to the opinion that mercury ought not to be used indiscriminately in all primary sores. In the true local disease it is not only unnecessary but hurtful, as it appears to us to retard the healing of the sore. We have not in these cases to deal with the poison

of syphilis, and therefore should not treat it as such. We have seen that this disease runs a definite course, and terminates without infecting the constitution. Our treatment, therefore, should be based on this principle. If there be no contrary indication, I believe the best practice to be at once to destroy the sore with strong nitric acid; keep the patient in bed so as to prevent irritation of the parts, which would tend to increase the chances of a suppurating bubo; support the constitution with a generous diet and ferruginous tonics; and in the very great majority of cases the patient will get well in about a fortnight or three weeks.

We must now pass to the inquiry into the evidence given on the second question. On this point forty witnesses also gave evidence. Twenty-one were of opinion that it was especially in the indurated form of chancre that mercury was most beneficial. The impression made on the minds of those who had thus treated the disease was, that it healed the sore, postponed the secondary infection, and modified it when it made its appearance. Very few were of opinion that it gave exemption from secondaries, but some were of this opinion. Among these was Mr. Acton, who stated that "he was sure that mercury gave exemption from syphilitic disease." Dr. Marston, of the Royal Artillery, believed that mercury and syphilis were antagonistic. Mr. J. R. Lane stated that he believed mercury tended in a hard sore to the absorption of the induration, and to diminish the liability to secondary constitutional affections; and he mentions a case where there had been well-marked induration for weeks, and mercury had been given, and no secondary affections had appeared for a very long time. Mr. Paget stated that "he regarded mercury really as a specific for syphilis in this sense, that, provided the patient is one who can safely take mercury, it will very materially shorten the duration of the indurated sore; and if it can be favourably received into the system, will prevent the occurrence of secondary symptoms." Nine witnesses did not recognise any particular form of sore in which mercury should be given, as they gave it in all. Five never gave mercury at all in primary sores; and four were non-mercurialists. One recognised only an ash-coloured form of sore in which mercury was useful; this was Dr. Stewart, Surgeon-Major in the Bombay Army, who stated that in India secondary affections most frequently accompanied this form of sore.

From the above statements it would appear that the majority of the witnesses recognised a sore in which mercury proved useful as a therapeutic agent, because, in addition to the eighteen who were decidedly of this opinion, the nine who did not recognise any particular sore in which it should or should

not be given, nevertheless were decidedly of opinion that it was beneficial in indurated chancres.

My own experience in a regimental hospital, where the opportunities of watching cases for almost any length of time are very great, has taught me that in the infecting form of sore there is but one safe practice, and that is, the continued administration of mercury in some one form or another. I indorse again Mr. Henry Lee's statement in his lectures on *Practical Pathology*, vol. ii. lecture xxxiv., where he states, "that it may be safely affirmed that the general experience has proved that no remedy exists possessing so great a power over the syphilitic poison as mercury." My strong impression, after watching a large number of cases, is, that in some instances (though I am far from saying in the majority) mercury does prevent the occurrence of secondary symptoms. At the same time, I believe I can state with perfect confidence that it does postpone the appearance of the secondary affection, and mitigates it where it does do so.

Having decided that there is a syphilitic sore in which mercury is decidedly beneficial, we must next inquire into the evidence given with regard to the form in which it should be administered. Twenty-six witnesses stated their opinions with regard to this. Eleven gave the preference to blue-pill, six to inunction, three to fumigation, two to mercury and chalk, two to Plummer's pill, one to the bichloride of mercury, and one to the protoiodide. The remainder of the witnesses either did not give any opinion on this question, or considered it immaterial in what form it was given. Of all the preparations of mercury that I have mentioned, I find that blue-pill has the greatest number of supporters; and my own observation leads me to give my vote in favour of blue-pill in the primary infecting chancre. I believe that in this form its effects are most easily managed. As a rule, it is well borne by the system, and it is a clean and convenient mode of administration. At one time I greatly preferred administering mercury by fumigation, according to Mr. Henry Lee's method, and which he so strongly advocates, and with which he has had such good results. Of late I have discarded it in the primary sore, as it appeared to me that the results were not so satisfactory as they are when mercury is given in the form of blue-pill. I believe that the ill results spoken of as the result of this drug are in a great measure due to want of care in its administration. While taking it, the patient ought, if possible, to be kept in bed, or at least in a perfectly equable temperature. The weight of the patient should be registered before commencing the treatment, and this should be repeated every third or fourth day. I am indebted to Mr. Cutler for

this latter suggestion, and it has proved to be a most valuable one in the treatment of syphilis. As soon as the mineral begins to have any pernicious effect on the system, it will be indicated by loss of weight, even before other well-known symptoms present themselves. The mercury should at once be remitted for a short time, a purge be given; and the patient will almost invariably regain the lost weight. So valuable an adjunct in the treatment of syphilis do we consider this, that in our regimental hospital a patient who is admitted with a primary infecting chancre is weighed at once, and the weight is registered every third day.

There is a third condition that I would strongly insist on, and that is, the combination of some tonic with the blue-pill; and I have found the sulphate of quinine to be most beneficial. My usual mode of administration is, three grains of blue-pill with two of the sulphate of quinine. If these precautions be carefully adopted, I feel convinced that the treatment by blue-pill will prove both safe and effectual.

We next pass to the consideration of the evidence given on the question,—Is mercury beneficial in the secondary evidences of the disease, or is there another therapeutic agent which gives better results either alone or in combination with mercury? The evidence given was decidedly in favour of good results being obtained by the judicious administration of mercury in the secondary sequelæ. Thirty-five witnesses gave their opinion on this point. Thirteen were of opinion that mercury alone should be administered; nineteen were in favour of a combination of mercury and iodide of potassium; and three only considered that iodide of potassium alone was sufficient to cure the disease. Of the thirteen witnesses who were in favour of mercury being administered alone, many considered that it was only in the tertiary stages of the disease that the iodide of potassium was of value as a remedial agent. The preparations chiefly advocated by these witnesses were the bichloride of mercury and fumigation. Those who recommended the use of iodide of potassium alone had but few followers, as experience had shown that, though a valuable adjunct to mercury, yet when given by itself it was disappointing in its results.

Mr. Henry Lee, in his Pathological Lectures, strongly advocates the calomel vapour-bath in secondary syphilis; and here I cordially agree with him. I believe, from my own observation, that in the eruptive form of syphilis there is no remedy like calomel fumigation. It cures quicker than any other mode of treatment, and the patient's system does not seem to be impaired by its use. With regard to the iodide of potassium, I believe that it does assist the action of mercury in this

stage of the disease; but it is in the tertiary stages that it becomes such a valuable curative agent in eliminating the poison. When given alone, it certainly does seem to get rid of the symptoms for the time; but they return in a short period, and are as troublesome and severe as ever.

It would appear, then, that mercury is the medicine to be administered in the secondary disease, and that there is no medicine which alone gives such good results; but that the iodide of potassium, in combination with it, assists its action very much.—*St. George's Hospital Reports, Vol.* v., *p.* 77.

65.—THE TREATMENT OF GONORRHŒA BY THE URETHRAL DOUCHE.

By Thomas Windsor, Esq., Manchester.

Injections have been used from a very early period, and many and most contradictory have been the opinions promulgated about them: they have been recommended for the very commencement, and again have been restricted to the very latest stage of the disease; some practitioners have advised that they should be very strong, others very weak; stricture has been attributed to them, and they have also been thought its best preventive. The usual experience seems to have been that strong injections, generally of nitrate of silver, have not, on the whole, proved satisfactory; they are extremely painful, often fail to do good, and have somtimes been followed by serious complications. On the other hand, in order that weak injections may act efficaciously in the earlier stages of the complaint, it is necessary to continually repeat them, every hour, even every half-hour—a frequency of application to which few patients can submit; the repeated introduction of the syringe, the distension of the inflamed urethra, and the mixture of pus with the vehicle can scarcely fail to do some harm.

Such reasons led me to consider whether some better mode of applying injections could be found than the syringe, and whether it might be possible to run a stream of cold water through the urethra without risk of inoculating its deeper portion. A few trials showed that there was no difficulty in effecting this so far as the three or four anterior inches of the canal are concerned. I used at first a vulcanised India-rubber tube with a piece of lead at one end and the glass cylinder of an ordinary urethral syringe at the other, the piston, of course, being removed; the tube was filled with water, and inverted into a jug full of a weak solution of permanganate of pot-

ash, so as to act as a syphon; the force of the current was regulated by the height at which the jug was placed. Thus the solution was allowed to run in a steady stream through the urethra for as long a time as seemed desirable; at first, after the glass nozzle had been introduced, only just sufficient pressure was used to keep it in its place, the fluid being allowed to flow freely back; in two or three minutes the glans was gradually compressed, so that the fluid passed further along the urethra before it was allowed to escape. The douche was continued for five or ten minutes at a time, and repeated a few times daily. The effect was to me satisfactory; the swollen penis soon collapsed, and the patient felt a sensible relief on each application.

In the few trials that I have had the opportunity to make, half a grain of permanganate to the ounce of water appeared to be the best strength. I have also used various amounts up to six grains to the ounce, but when increased beyond two or three grains, the application caused pain, and seemed to take less effect on the discharge. As a general rule, the more acute the inflammation, the weaker should be the solution.

As the syphon is somewhat troublesome to use, I have latterly replaced it by the ordinary India-rubber enema ball and tube, the ivory end being replaced by a glass cylinder. This modification renders the process very easy. Of course, if the douche is frequently wanted, still simpler apparatus may be employed, such as a tube with stop-cock fastened to the bottom of a can, or in towns a pipe simply fastened to a water-tap would answer for water alone.

I have made a few trials to wash out the membranous portions of the urethra by using a catheter in place of the glass nozzle, but without success. Probably it could be effected by a double tube, but I have not thought it necessary to proceed further, for if the symptoms of acute inflammation in the anterior portion of the urethra are overcome, those in the deeper parts rapidly disappear as the rule.

After a time, when the discharge is much diminished, and unattended by pain, stronger injections of lead, sulphate of zinc, &c., are called for, and the ordinary urethral syringe answers every purpose.—*Manchester Medical and Surgical Reports, Oct.* 1871, *p.* 54.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

66.—ON THE PROPER MANAGEMENT OF TEDIOUS LABOURS.

By Dr. G. Hamilton, Falkirk.

[Dr. Hamilton published a paper in 1853, in which he stated, that out of 317 consecutive labours attended by him, all the children had been born alive with the exception of one. Rather more than one-eighth of these had been delivered by the forceps. The one case was a breech presentation. Since that period Dr. Hamilton's experience has been still more favourable, having had 731 children born alive successively, the 732nd case being still-born, and that a footling case.]

Looking back on my practice for forty years in this department, one of the most striking of its late characteristics has come to be, it appears to me, its simplicity. A mere enumeration of some of the negations that have occurred in it will show this well. For example, 1st. I never now use bleeding or antimony to relax rigidity of the os uteri. 2nd. I never now, except in special cases, use secale cornutum to hasten labour. Only a few times during the last thirty years have I required its assistance. 3rd. I now rarely, if ever, interfere with the first half of labour. 4th. I now never require my patient to have supports for her feet, or a pillow between her knees. I simply ask her to lie on her left side, and keep her knees well drawn up; and I have only occasionally, even in applying the forceps, to ask her to shift her position to the front of the bed. 5th. For thirty years I have not I think once, in my own practice, had occasion to use the catheter. 6th. During this period I have not, in forceps cases, had occasion to ask the assistance of any of my professional brethren. This I mention, not certainly in a spirit of self-sufficiency, but for the purpose of showing how comparatively easy my practice has been. 7th. I never grease the forceps before introducing them, as I think this tends to make them slip. On the con-

trary, I strongly recommend that the inner surface of the blades be slightly smeared with India-rubber paste, which soon dries, and enables them to lay firmer hold of the scalp than does the bare metal. 8th. I never now use forceps with a double curve. All my cases, during the period alluded to, have been delivered with Ziegler's straight forceps. 9th. I never now, in my own cases, see the parts of the mother injured more than in an easy natural labour. As for rupture of the perinæum, it never happened to me in all my practice. 10th. Although I do not absolutely refuse the use of chloroform, I use it as seldom as possible, and in certain cases decline to give it as inadmissible. I generally require in tedious labour the full power of the uterus, and I have found that chloroform often deprives me of this. 11th. I never, if I am able, apply the first blade of the forceps otherwise than over an ear. 12th. My forceps have no notches on the handles, for tying them, as I entirely disapprove of the practice. 13th. And finally, it is pleasant for me to be able to say that, as a result, I almost never have still-born children.

On several of these points I shall have to speak more fully as I proceed.

Having premised these negatives, it will be found, as I have said, that the management of the two stages of labour is thus considerably simplified. By the first stage, I mean from the commencement of the pains till the full dilatation of the os uteri, and the entry of the head into the pelvis; by the second stage, from this till the completion of labour. As I have said, I now rarely attempt to interfere with the progress of the first stage of labour, even when this is protracted for some days. Indeed, when I can, I keep as much as possible out of the way of my patients; recommend them to walk about or lie down, as they may incline; to take a little sherry and water to support the strength; and, in fact, I get over it the best way I can, without interference. As is well known, the late Professor Hamilton laid great stress on not allowing his first stage to last more than from twelve to fourteen hours; and I can recollect we, his students, were particularly active in following out his directions, never going to a case without a supply of ergot in our pockets, and pestering our patients with our officious endeavours to hurry on the labour. All this I now believe to have been unnecessary and hurtful, and I imagine most of the profession are agreed with me.

Professor Hamilton and Dr. Burns approved of supporting and dilating the uterus; Dr. Ramsbotham did not; and Dr. Murphy, as far as I know, still holds the same opinion. I have in this matter always acted up to the instructions of my old teacher; and from the time I began to use the forceps

more frequently than was usual, I saw that the importance of assisting the ascent of the uterus was increased, for until this obstacle has been removed, the forceps can never be used with freedom and safety. It may therefore properly be said to be (although most necessary I think independently of this) a preparatory step to their application. I have, in thus dilating, encountered many rigid os uteri, but have rarely failed in accomplishing my object, by first giving plenty of time in the first stage for the parts to become properly prepared; and, in the second place, by using, when necessary, pretty determined force in the second stage. In doing this I have never met with a case where any unpleasant accident occurred to the uterus, from tearing or otherwise, and the little extra pain inflicted on the patient I have usually found well borne, where she has been assurred that this was necessary to help on the labour. Where the os uteri has been forced back towards the promontory of the sacrum, and has been kept there from the head pressing the uterus on the pubes, which latter in such a case may be found to project more or less inwards, or where, on the contrary, the head and promontory of the sacrum catch the uterus, or where, again, this is done—as sometimes happens—by both pubes and promontory, then I set myself to find out what is the exact cause of the detention, and introducing the hand more or less fully, determinedly push the uterus over the head. When this has been effected, I generally consider the rest of the case comparatively simple. I was called to a labour not long ago where a midwife had worked at the case for twelve hours without getting the uterus over the head. When I saw the patient the os was perfectly expanded, but the uterus was caught by the head on both the pubes and promontory of the sacrum, and I had to work hard for two hours and a half more before I succeeded. As time was precious after such long detention and hard work, I immediately applied the forceps and delivered—mother and child being perfectly well next day. Now let us for an instant reflect what might have been the consequences had the active measures used been neglected, and the labour had been protracted some eight or ten hours longer. The uterus, thus caught, must infallibly have become swollen and inflamed, and have blocked up the passage; and this in all probability becoming what has been called a "long forceps case," the instrument would have had to be applied under the most disadvantageous and difficult circumstances, the child very likely being lost, and the life of the mother endangered.

The more I investigate this point the stronger is my conviction that it is in many cases second in importance only to the use of the forceps. I am quite convinced, from my constant

experience, that the excessive dread of many practitioners of injury to, or subsequent inflammation of, the uterus, in doing so with caution, and yet with firmness, is unfounded. At all events, I have not seen even a single case which has made me doubt its safety and propriety; and I can join with those practitioners in its condemnation only when it is used in the *first* stage of labour, which I have no doubt was formerly too much the case. I am inclined to believe that it is now becoming so clear as almost to be axiomatic, that while the safety of the child demands a more frequent application of the forceps than has been customary, the safety of the mother as decidedly demands that these should be applied as seldom as possible *within* the uterus. Convulsions, or some other exceptional occurrence, may imperatively require this; an ordinary labour, if it has been well managed, I should say hardly ever does.

It is very interesting and important for me to state, as having a connection with this subject, that I have never, in all my own practice, had a case of laceration of the neck of the uterus; and I believe the reason of this has been my constant and anxious endeavours to get the uterus over the head. In looking over the former numbers of this journal, I find, in 1851, a notice of Dr. Roberton's "Essays on Practical Midwifery," in which it is stated that he himself had met with ten cases of this kind, in which seven of the females died; and he has collected from different sources seventeen other similar cases. The reviewer says, "The sign of impending danger in these cases, which Mr. Roberton thinks the most pronounced, is a feeling of crampy pain, and tenderness on pressure, on some particular part of the lower abdomen; and he explains the cause of the crampy pain by referring to a case in which it occurred as the result of the cervix being held by a 'vice-like grip' between the head and the brim." The practice which Mr. Roberton counsels, if there is space for the head to pass, is "to watch the case attentively, to apply a binder, and, *perhaps*, to raise the caught lip of the uterus," &c. In such cases of catching of the uterus I have had no hesitation as to the practice to be pursued. I *must*, if possible, get the uterus over the head, and, as I have stated, I have very rarely indeed failed in doing so.

It is with the commencement of the second stage of labour, therefore, that our active interference should generally begin, if we be called on to interfere at all. If the membranes have not been ruptured, and the head is presenting fairly, I then do so at once, and gently but firmly continue to press up the uterus with one or two fingers, or with the whole hand if necessary, until I have got it pushed over the head; and, as previ-

ously observed, I like to use the forceps as seldom as possible before this has been effected. As this is especially necessary should ulterior help be needed, I never neglect it, and work assiduously at it till it has been accomplished, if possible. Sometimes I find myself unable to do so in a moderate time, and I then make up my mind to use the forceps, or other means for delivery, under comparatively disadvantageous circumstances. In my first paper in this Journal I gave it as my opinion that it is not safe, for the child especially, to allow this half of labour to continue much more than two hours, and I still adhere to that rule, as being of the very greatest importance. Much, more or less, as to interference, may depend upon the kind of labour we have to treat, but, as an average, I am convinced more than two hours cannot be trusted to; and in special cases, to secure a favourable result, we must shorten this time very materially.

It is curious to note how Dr. Murphy tries to evade the necessity for interference with the forceps in relation to this point. He says, in this "controversy a new and very important question has been raised by Professor Simpson, which, if true, would decide in favour of interference in all such cases. He has shown from statistics that the mortality is increased in direct proportion to the length of the labour; that a labour of four hours' duration is more fatal than one of two hours, one of eight hours than one of four, and so on. Hence the inference that protracted labours are dangerous because of the *time* they occupy. We have given this important question the reflection it so justly merits, but confess we cannot coincide in the conclusions drawn from it. It seems to prove too much, that not only are the longest labours the most dangerous, but that the shortest are the safest; neither of these propositions has the support of our experience. The danger of protracted labour depends upon many causes; and if the constitution be good, *time alone* is the least injurious. Rapid labours are attended with risks from which those of moderate duration are free. We do not think therefore that the shortest labours are the safest, or the longest *in time* the most dangerous. The question must be determined by individual experiences." No doubt Dr. Murphy is correct to a certain extent in this, and Sir James was clearly wrong in holding, from his statistics, that the risk to the child is in the ratio of the length of the labour *as a whole*, as I endeavoured to show in my paper in this Journal in 1853. But, granting this, will Dr. Murphy deny, or is there an experienced accoucheur existing who is not profoundly impressed with the conviction, that the ratio of mortality to both child and mother, but especially to the former, is most intimately connected with the duration of the *second*

half of labour? And still further, after something like two hours, that the danger increases with every additional hour, not in a simple but in a very serious compound proportion?

Holding these views, and the principle I have stated, as vital, I never delay in head presentations the application of the forceps, and I find not the slightest difficulty with my patients in doing so. As I have said, they or the attendants have never to be alarmed by extra assistance being required, and they are never alarmed by the "instruments" being sent for. If the case is at the least distance, I always have the forceps in my pocket; if near, I quietly slip out and provide myself with them, when I see they are likely to be needed. Generally, some time before applying them, I point out to patient and assistants that the pains are not doing good, or I tell them that the position of the head requires a slight rectification, and then, without the slightest fuss, I apply them and deliver. Sometimes this is effected immediately; in other cases longer time is required; and in the more difficult ones I take a good while before I succed. The forceps will sometimes be applied and a few pains assisted; then they will be taken off and the patient encouraged to take a few pains by herself; then they will be reapplied, perhaps several times, before I get complete power over the head. My plan always is to assist, and not to supersede nature. And here I may mention the superseding of nature as one of my objections to the use of chloroform. One of the chief difficulties I have encountered is in getting a good hold of the head, from its being too high up in the pelvis; in such cases smart pains are of great importance in bringing it within reach, the forceps being already introduced and ready to lay hold of it. But chloroform often dulls the pains, and in this way presents an obstacle to our operations; in general, therefore, I avoid it. For exactly the converse reason, it is in these cases, and in these alone, that I have used the ergot, except at the close of labour to prevent flooding. In using it to effect the purpose I have in view we run a certain risk of killing the child, but when the delivery can afterwards be effected quickly, it is, perhaps, good practice to do so. As I have already said, however, I have required to run this risk very rarely indeed. The danger of using it over and over again in the *first* stage, or indeed in either stage of labour, I think very few will now be inclined to dispute.

The cases I have delivered with the forceps I would divide into four classes; the first being those where the head is well down in the pelvis, an ear easily felt, say near the right acetabulum, with the face to the same side. These, almost invariably, I have found it remarkably easy to manage. Gentle traction is applied, the head comes still further down, and the face

passes into the hollow of the sacrum, sweeps along it, and the child is delivered. And yet, simple as they are, my belief is, that in hospital practice, among timid practitioners, and in the hands of midwives, it is in this class that the principal part of the mortality to the children will be found. The case probably is lingering, but "everything is fair," the patient is not exhausted, &c.; valuable time is lost, and the child is stillborn.

Once more, in concluding this part of the subject for the present, let me give the following extract from my note-book, dated May 2, 1871:

"Was called this morning at 8.30 a.m. to Mrs. B., multipara. The membranes had ruptured last night, and a midwife had been with her since 3 a.m., who stated that the labour had been moderate, but was now making little progress. I found the head well down towards the perinæum, an ear easily felt, and a small caput succedaneum. The patient had become uneasy and restless. I applied the forceps, and delivered, with the assistance of three pains, the cord being found round the neck, and short." On the above notes I make the remark, that I should have liked some of those practitioners who used, not long ago, to apply the forceps once in 500 or 600 times, to have been present and seen the ease with which this really trifling operation was gone through, and the safety and smiling comfort it at once brought into a household. They would, indeed, in such a case, in my opinion, have been "obstetrical reprobates" (the phrase is not mine, but Dr. Murphy's, which, he tells us, he quotes from Dr. Blundell, *with approbation*, which I certainly do not) if they had failed to see and admit the vast benefits which the more frequent use of the forceps has conferred, and is likely in the future to confer, upon humanity. By no other operation that I know of, except, perhaps, vaccination, can such a saving of human life be effected, and in the great majority of instances I have found the performance of the one operation almost as simple as the other. If we take the annual births in the United Kingdom to be about 1,000,000, a lessening of the infantile mortality in these by only one per cent. would give us a saving of infant life in each decennial period of no less than 100,000.—*British and Foreign Medico-Chirurgical Review, Oct.*, 1871, *p.* 449.

67.—ON RESTORATION OF THE PERINEUM.

By Dr. J. Matthews Duncan, Edinburgh.

[The term restoration of the perineum is not altogether free from objection, inasmuch as the operation has sometimes to be performed when the perineum is anatomically intact, but functionally imperfect. The cases in which this operation is

absolutely necessary are those in which the whole sphincter is destroyed; but if the sphincter is only partially injured its action is impaired, and entails a loose condition of the bowels, almost amounting to diarrhœa.]

In cases of procidentia, whether the perineum be anatomically entire or not, it is always functionally imperfect. It does not prevent the passage of the procident organ or organs over it and through the vaginal orifice, as it might do. It is gradually distended, the vaginal orifice is dilated, the perineum or its remnant is pushed backwards, and its antero-posterior length is more or less completely annihilated. In this way, cases which begin with an entire perineum are at length brought, not anatomically, but functionally, into the same condition as those in which the perineum was previously extensively ruptured. When the procident parts are pushed back, and the perineal region is exposed, there is found a very unnatural condition of parts. Instead of the labia majora being in contact, or nearly so, there is seen a large rounded gaping vaginal orifice, generally closed by the rugous anterior vaginal wall, and extending from the clitoris in front to the narrow transverse strip of perineum behind. This condition cannot but cause symptoms, and accordingly the woman describes a feeling of disagreeable openness. This gaping orifice the surgeon closes by restoring the perineum, making a new and long perineum anatomically, with the hope that it will efficiently assume the function of closing this orifice, so as to prevent the return of the procidentia. After the operation is finished, and again after the cure is complete, the surgeon finds, when he exposes the perineal region, a quite different appearance. There is now no gaping orifice; no mucous membrane is visible; the labia majora are in contact—indeed they are extensively united.

The operation which I now propose to describe is very generally successful. Success, indeed, may be counted upon. It is rarely that a second operation is required. I have operated on cases for the second time, but not in any case which was from the first under my own care. Previous failure does not prevent future success.

When the operation is performed for injury of the sphincter, the woman feels her renewed power to restrain flatus and fæces however liquid. This renewed power is not in every case complete; for it occasionally happens, that although there is great improvement, and consciousness of regained sphincteric power, yet the recovery is not quite perfect; the control over feculent and flatulent evacuation is not in its pristine perfection.

When the operation is performed for the cure of procidentia, the cure is complete or the failure is complete. This does not imply that a bandage is not a judicious and advantageous appli-

cation to aid the restored perineum to do its work of restraining the advance of the organ which tends to fall out. The cure is complete, I have said, and it is a cure of which the surgeon may be proud. The cure is not only often complete, but also permanent. In illustration I may cite a case, and it is not of a rare kind. A multiparous woman, whose employment keeps her almost constantly standing, walking about, stooping, and lifting weights, had a large procidentia. She was operated on and cured by a colleague. The cure lasted a long time. She had again a child. The cure still lasted. Again she had a child and soon after this the womb came down as badly as ever. The restored perineum was still anatomically entire; the cicatrix of the first operation could be traced; but its restraining function was lost: it was thin and relaxed. The operation was repeated. About three weeks afterwards she returned to her arduous employment, and was and now is quite cured. As her husband is dead, it may be hoped that the cure is for life.

What is the proportional number of failures and of successes after this operation I cannot tell. It is a proceeding to which I frequently resort, but I have found it impossible to trace the subsequent histories of the great majority of my patients. The sufferers from aggravated procidentia are for the most part very poor women, whose hard lives and frequent changes of residence form some excuse for their failing to fulfil promises to keep the surgeon informed as to how they are "getting on."

I have just mentioned a case in which the restored perineum continued entire after two births at the full time. The retaining function of the perineum was, in that instance, destroyed, not by laceration, but by distension. I have repeatedly seen cases in which the birth of a mature child did not extensively or injuriously lacerate a restored perineum.

The operation runs great risk of failing from disorder of the healing process, the opposed raw surfaces secreting pus instead of uniting, if the subject of it is very old, if she is of syphilitic constitution, or if the parts are the subject of recent or of chronic inflammation. Of course, under these conditions, it should not be attempted.

In cases of laceration of the perineum the operation may be undertaken at the time of the accident or after the injured parts are quite healed. I have done it successfully at both times, and if circumstances are all favourable I would recommend its performance at the time of the accident. Yet I think a successful result can be more securely predicated of a delayed operation than of an operation done at the time of the injury. It is to be remembered that the perineum often appears to the inexperienced to be extensively injured during labour, and so as to demand operative interference, when truly very little harm

has been done; as is made apparent by examination after the parts are healed.

The instruments required for the operation are the following:—a sharp bistoury, a dissecting forceps, a catch forceps, ligatures for arteries, needles armed with silver wire for stitching, and a scissors. Besides these, the ordinary appliances for all operations must be at hand, as sponges, &c.

The operation should be done soon after a monthly period has passed; and, in preparation for it, the bowels should be freely evacuated. On the morning of the operation the patient should have only soup or some light nourishment, in order that the vomiting so frequently accompanying and following the induction of anæsthesia, which is maintained during the operation, may be kept within the narrowest bounds.

In describing the operation, I shall suppose the case to be one of laceration of the perineum, the fissure extending through the sphincter ani. It will be unnecessary to go over the steps of the operation for procidentia, because, *mutatis mutandis*, they are quite the same as in a case of laceration.

The patient is placed and held in the position for lithotomy, and the surgeon is placed as he is during that operation. With his fingers or with forceps he seizes the fourchette, or that part which corresponds to it, transfixes it with the bistoury, and then continues to cut, first on the one side and then on the other, upwards from the fourchette or its representative as far as he deems necessary. In cases of procidentia it is usual to make the raw advance as far forward as to be nearly on a level with the orifice of the urethra. The operator thus removes a long tape-like piece of integument, which is about half an inch broad, rather less than more; and he leaves a horseshoe-shaped wound in which the point of the shoe is at the fourchette. It is important that the proper piece of integument should be removed, and it is at some parts a matter of care to secure this. At the fourchette there is little difficulty. It is quite easily made out, or may be made to project by separating the labia. Farther forwards on the sides of the vaginal orifice, the junction of skin and mucous membrane is sought for as the line of the wound, and it is not always quite easily found. The anterior margin of the wound is at or involves the posterior extremity of each nympha, and the line of the wound runs between this and the fourchette, its course being sometimes marked by the opening of the duct of the vulvo-vaginal gland on either side.

Generally two, sometimes four, arteries require ligature. Two of these are in front of the sphincter, one on either side; the other two are generally further forwards near the nymphæ. A few minutes' delay is now caused by waiting for the complete or nearly complete stoppage of oozing from the raw surface. Then the wound is closed.

A series of silver wire sutures is passed about one-third of an inch apart. The wire sutures, after being placed, are observed to pass through the wound near its deeper margin, and emerge on the skin or rectal mucous membrane about a third of an inch distant from the outer margin of the wound. Beginning posteriorly, each suture is tied with some firmness, the edges of mucous membrane and of skin being carefully adjusted to one another. Now the bladder is evacuated, the vulva is washed and dressed with some wet lint, and the operation is finished.

The alvine evacuations are stopped for about ten days by daily use of opium in some form. I use solid opium in one-grain pill at bed-time, or oftener if there appears need for it. The patient is fed on light food and sparingly. The urine may be drawn off twice or thrice daily, or the patient may herself make it while lying. The wound requires to be kept clean by daily dressing. Care should be taken that discharge does not accumulate in the vagina.

On the seventh or eighth day after the operation, the stitches are removed. Twenty-four or forty-eight hours afterwards the bowels, if they do not spontaneously move, are acted on by castor-oil.

The removal of the stitches is done as in the operation for vesico-vaginal fistula. A dissecting forceps is made to seize the projecting end of the ligature, and to tighten the loop by traction; then one blade of a sharp-pointed scissors is insinuated within the loop, and the scissors is made to cut it; after which it comes away by the traction of the forceps. But in a case of extensive perineal laceration it is sometimes difficult to get at the deepest or farthest back sutures. They may be half an inch or more within the margin of the new anus, and require considerable care in dealing with them.

Very many different plans for performing this operation have been proposed and practised; and of these many involve proceedings of which I disapprove, such as removing large portions of vaginal mucous membrane, and incising the sphincter ani. The plan I have rapidly sketched has the recommendation of being as simple as possible, and I have had such considerable experience of it as to justify my preferring it on the best grounds as being at the same time very successful and satisfactory.—*Edinburgh Medical Journal, November* 1871, *p.* 385.

68.—PREGNANT SICKNESS.

By Metcalfe Johnson, Esq., Lancaster.

In considering the effects of remedies upon the human body, the possibility of error in some form or another presents

itself so frequently as to induce great hesitation to accept coincident results as necessary consequents. There are, however, some drugs, such as opium, whose effect in sleep is seldom doubted. The point to which I desire to direct attention is one which is beset with difficulties. My object now in writing is to invite consideration for the phosphate of lime as a means of relieving the sickness consequent on the pregnant condition. But when we consider the double relation of mind and body, through the ganglionic nerves and the disturbed state of their functions, in all cases in which the control of the ganglionic nerves, or the great sympathetic, is interfered with by an abnormal condition of organ or organs under its especial rule, we shall see that it requires especial watchfulness to be sure that we do not mistake a "*post hoc*" for a "*propter hoc*" in those instances where relief of symptoms has followed the exhibition of the remedy. Before proceeding to remark on the theory of *modus operandi*, &c., the simple use of the drug may be described. For some years past I have been in the habit of prescribing the simple hydrated phosphate of lime of the Pharmacopœia in doses of from three to ten grains each, three times a day, suspended in water, and flavoured according to the taste of the patient. I have tried the remedy dissolved in hydrochloric acid, as also the powder in the dry state, besides having had it made up into biscuits; but in none of these forms have the same agreeable results followed so frequently as when the simple hydrated phosphate has been used suspended in water.

One remark may here be made respecting disorders of the great sympathetic and its subjected organs, that they are generally characterised by a dislike of all sweet flavours. This has been noticed in the case of persons whose ganglionic system is disordered through the stomach by the abuse of alcohol; for I think it is Coleridge who says there is always hope for a man so long as he is fond of his pudding. But in those other forms of female ganglionism which, for want of a proper diagnosis, we designate under the generic term hysteria (though in many cases the ὕστερον has nothing to do with it,) the taste not only takes an aversion to sweet things, but has, apparently, a depraved tolerance of the flavour of fetid preparations and the alkalies, together with an ability to receive the stmulus of both alcohol and the carminatives, such as lavender, ammonia, cardamoms, &c., with advantage. The relation of this sympathetic nerve to certain conditions of the circulation is a subject worthy of more attention than it has at present received; more especially since the *Saturday Review* has, with a one-sided view of the matter, taken up such a raid against "alcoholism."

With these complex considerations we approach the subject

of pregnant sickness or vomiting. Here we have, of course, an enlarged uterus, which physically bears a relation to the various organs of the body, such as the stomach, large and small intestines, liver, gall-bladder, kidneys, &c., different from that in health, pressing upon each, and producing a state of things with reference to each organ which, if brought about by traumatic means, would in any case bring on nausea and vomiting. But, in addition to this, the very relation to the nerve is altered, and in many cases this great change is attended with not only bodily suffering but temporary mental aberration. I have not unfrequently seen temporary insanity of a few hours' duration attend both the act of conception and the act of quickening. Everyone is of course familiar with the puerperal insanity as well as the frequent insanity which is associated more or less directly with ovarian or uterine disease.

A short time since, Mrs. A. B., aged 24, second pregnancy, during the last month has had violent spasms all over the body, with strabismus. Mouth and hands clenched. Says she has pain all through the womb. Has great sensibility in the nipples of the breast. Bowels generally confined. Has had bleeding at the nose for the last few days. Her mother is a very excitable, clever woman, and one other close relative a somnambulist. The spasm was relieved by subcutaneous injection of morphia. I emptied the bowels by an aloetic enema, and gave her the phosphate of lime, which she took for three weeks, after which she was delivered of a very small child, the parietal bones of whose head consisted simply of two centres of ossification. Since her confinement she has been well. The spasm *never* returned after using the phosphates. I have often had this proof of the efficacy of the phosphates in arresting the sickness: that patients have been sent to me for "some of that medicine that relieves the sickness."

I had a patient a few weeks ago, who had been complaining for some weeks of an irresistible vomiting after every meal, who no sooner took the phosphates than all sickness ceased. This of course might be the effect of expecting to be relieved; but the cases have occurred too frequently for me to think other than that relief has been most frequently the result of the use of the phosphates. As such, I trust that some of your readers will be induced to give the remedy a trial, for which I now proceed to give a physological reason or *ratio medendi.*

As we have seen, the altered shape of the uterus, the altered nerve relations, the control of the ganlionic nerve to supply the new arterial system to be established, make a demand upon nervous influence which is very unusual. Nervous power cannot be expended without harm, unless the supply of new neuric elements makes up the deficiency. Neuric force derives much of

its nutrition and source from phosphates. Moreover, the child in its formation requires more phosphates for its new bones, and if these are supplied at the expense of brain and ganglionic nerve, it follows, as a matter of course, that debility, nervousness, and all the concurrent train of symptoms must inevitably be brought about; and hence arise those feelings of depression, peevishness, and irritability so frequently associated with the pregnant state. Nor is it to be wondered at, if we consider that we take no steps to supply the new demand made upon the blood.

This view of the case is again supported by pathological evidence, when we see how that fractures in pregnant females are more frequently liable to non-union. These considerations induce me to believe that the remedy is really the cause of the relief so constantly expressed by the patient after its use for a few days. I have also for some years been in the habit of using this form of phosphates for the relief of rickety children with great success, which further confirms me in the belief that phosphates administered through the stomach do become used by the blood. I have used for children the saccharated wheat phosphates supplied by the druggists, but in the case of pregnant females I have not found these so useful, and chiefly I suspect, owing to the sugar which they contain.—*Medical Times and Gazette, July* 1, 1871, *p.* 7.

69.—ON THE MODE OF INTRODUCING THE MIDWIFERY FORCEPS; WITH NOTE ON THEIR MODIFICATION.

By Dr. James Cappie.

[In the following paper the writer draws attention to a single point in the management of the midwifery forceps. In works on the subject the instructions given are almost stereotyped, so much so, that it might be supposed there could be very little opportunity to suggest anything novel.]

The instruction I have to criticise is, that in introducing either of the blades, it is to be held lightly in the right hand, and that two or more fingers of the left, introduced into the vagina, should guide the point of the instrument, and guard the parts of the mother; and the amendment I have to submit is, that the task of introducing, guarding, and directing, should be trusted entirely to the fingers of the right hand, and that, in the first stage of introduction, the left, crossed over the right wrist, should have little more to do than to support the shank or handle of the instrument. The advantages of this plan are that the ordinary obstetric position does not require to be disturbed, that in many cases the instrument can be used

positively with greater facility, and, at all events, that in this way it can be applied in instances where it would be almost impossible in the ordinary way.

At the outset, I must mention that my remarks are intended to apply to the double-curved instrument. My experience of the straight forceps has been limited, and, so far as it goes, decidedly unfavourable. In leverage power, and in the thorough command we have over it, it is greatly inferior to the long forceps.

The first objection I have to the ordinary instructions is the awkward position in which we are directed to place the patient. She is to lie as much as possible right across the bed, and, according to some writers, the nates are to be made to project over its edge. To put her in this position, of course involves trouble to the patient, and all the anxiety to her and her friends that attends preparations for what is regarded as serious operation. It will not be denied that, when it has been determined to use instruments, the less formidable the procedure can be made to appear to the patient and her friends the better. Other things being equal, that plan should be adopted which can be gone about in the quietest manner. The position across the bed, however, is quite necessary if two or more fingers of the left hand are to be introduced into the vagina. When the perineum is relaxed the ostium vaginæ dilates, not from before backwards, but more nearly in the direction of a line drawn in continuation of the symphysis pubis. The twisting of the left wrist, therefore, would be extremely awkward if the body of the patient is not kept well forward.

With the plan I have followed, and would now advocate, the ordinary position does not require to be interfered with. The patient simply requires to be conveniently near the side of the bed. In very many cases it is a matter of indifference to myself whether she be lying on her back or on her side. If she is not under chloroform, and is anxious to facilitate procedure, I prefer a posture between the two. The right thigh can thus be more readily managed than when she is lying fairly on her side.

In regard to immediate management, we may take one of the simplest cases, where the head is at the outlet and the perineum is relaxed. An examination being made in the ordinary way with the right hand, the blade to be applied first—that is, in most cases, the lower or left iliac blade—is held in the left hand (which, as I have already said, is crossed over the right wrist), and the point directed to the margin of the outlet. If the perineum be well relaxed it will at once slide within the vagina, along the side or inner surface of the finger. Or,

allowing the middle finger to remain within or at the edge of the vagina, the forefinger is made to catch on the inner rim at the rounded end of the fenestrum, and to draw the point cautiously onwards. If the patient be stout and the perineum thick, a little care is necessary to prevent any fold of skin or mucous membrane being caught by the edge of the instrument. One or other of the fingers feels cautiously round while the point is being coaxed onwards; for, of course, through all the stages of introduction and adjustment, any great amount of force is out of the question.

The more firm the perineum is, the more necessary is it for the handle of the instrument to be directed well forwards. If the outlet of the pelvis be contracted, the handle may with advantage be even inclined to some extent across the left thigh, so that it is made to describe a complete half-circle as the blade is being made to sweep round into its proper position on the head.

When the point of the instrument is fairly within the vagina, its progress and direction are at first to be trusted almost entirely to *the forefinger pushing on the rim of the fenestrum.* I believe those who have never tried this plan will be surprised to find how easily it can be urged onwards, and how safely the proper direction can be given.

Of course, the direction for the first blade is, as a rule, first backwards along the perineum towards the sacrum, and then downwards and forwards, so that the outer surface of the instrument is toward the left ilium; and for the second blade, it is first backwards and then upwards and forwards, so as to lodge it between the child's head and the right ilium.

As long as the rounded point of the instrument is within reach it is pushed onwards by the forefinger, and the left hand has scarcely anything more to do at this stage than to allow the handle to rotate, and to be elevated or depressed according to circumstances. When the finger has pushed it onwards as far as it can reach, the left hand becomes the propelling force. The forefinger, however, still acts at any convenient point as a fulcrum to assist in giving the proper direction to the blade, while the hand gives the rotating and forward movement. If the os uteri has previously been slipped over the child's occiput, there is usually no difficulty now in at once adjusting the blade to the proper position on the head.

In this process there is wonderfully little risk of any injury being done to either mother or child. Ramsbotham indeed remarks, that if "a circular sweep of a portion of the pelvis is made, the maternal structures might be endangered." But it is really difficult to conceive how such an accident could happen, except by the grossest carelessness. When the perineum

is at all distended, the surface of the vagina is so smooth that it would be difficult, even intentionally, to make the edge of the instrument to catch on any fold of the mucous membrane. If the head is high in the pelvis the probability of doing so is still small, and may be avoided by the most ordinary care. I have applied the forceps in the way now described for twenty years, and have never met with any accident to the maternal passages by the sweep of the instrument. In regard to the progress of that part of the blade which is out of reach, the operator has first to have a clear conception of the direction he wishes it to take, and then to proceed with the caution, but, at the same time, with all the confidence that the surgeon has in passing the male catheter.

If the os uteri is still felt all round on the head, the *tactus eruditus* of the forefinger comes to be of great assistance. The instructions usually given to introduce the fingers of the left hand within the os, and to feel for the child's ear, appear to me to be not only needless but to be cruel to the patient, and indeed the attempt must often be difficult for the operator. The forefinger by itself, being intimate with the topography of the localities, can at once determine the position of the instrument relatively to the passages and the child's head. It not only takes up greatly less room, but it is positively more efficient. It is usually a very simple matter to introduce the point of the blade within the os, especially if the latter has been pushed low into the pelvis. When it is not easily reached, the method of procedure resembles what I have detailed for introducing the blade into the vagina. First, the finger tilts on the os, while the left hand cautiously advances the point of the blade till it rests on the child's head. Then the finger hooks itself into the fenestrum, and, if possible, keeps the point sliding along the scalp till it catches the lip of the os and is pushed within it. The part at which I prefer to enter the instrument within the uterus is, for both blades, at the posterior or sacral portion of the os uteri.

If the point has passed the os, and we are uncertain whether it is outside or within the lip, all that is necessary is to draw back the instrument slowly while the finger is resting on the os. If it be within the lip, it will be caught on the tip of the finger, and no hesitation need then be felt to urge it steadily onwards, and to give the necessary rotatory impulse to bring it into position.

It may sometimes be an advantage to be able to apply the forceps although the os uteri is not completely dilated, as in cases of severe convulsions, when uterine action is completely in abeyance, and the only hope for the mother is in the speedy termination of the labour. Now in the manner I have described,

the forceps may be passed into the uterus when the os is scarcely more dilated than just to admit the breadth of the blade. On this point, however, I shall not at present dilate, as I have brought it before the Society on a previous occasion.

In regard to the introduction of the second blade, the same general management is to be attended to. Introduced in front of the first blade, it is directed backwards along the perineum, and then upwards and forwards towards the right ilium. Here, as before, if the head be low in the pelvis, no difficulty is experienced in introducing it within the vagina, and in adjusting it on the head. If the outlet is contracted, the handle may be inclined across the right thigh, at the first stage of introduction, and a complete half-circle be described in the adjustment. If the perineum is thick, or the patient stout and with sturdy muscular thighs, a convenient mode of introducing the second blade is to project the handle so well forward that the left hand can support it in front of the thighs. When the patient is lying on her back, it can thus be passed with great facility. The forefinger again acts as the propelling agent in the first stage; and, according to circumstances, it does so either behind or before the shank of the first blade.

There is one point which requires a little study in the process of adjusting the second blade. Very often, when it is being made to sweep round to its position on the head, the handle is apt to be caught on the mattress; and this may sometimes be to an extent to make it appear to be impossible for the sweep to be completed. Usually a little coaxing will overcome the difficulty, but if this is not sufficient, all that is necessary is to get the patient to lie a little more on her back, and the handle may then be made to go round without interruption.

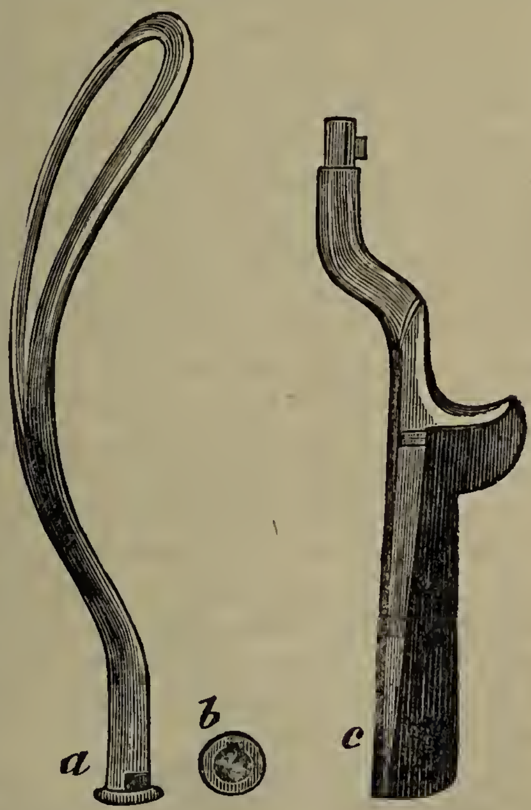
In regard to the locking of the blades I have nothing new to suggest. In most of the cases they lock readily with the first attempt. When they do not do so easily, an examination must be made to ascertain whether one of them cannot be better adjusted. I believe it will be found that if one of the blades inclines to fall easily out of position, that is the one which requires to be further introduced or rotated. It is seldom necessary altogether to withdraw either blade.

If the adjustment cannot be improved, some force may be employed to complete the act of locking. The shanks are brought into contact as near the lock as possible. Then each handle being held firmly, the one in the right and the other in the left hand, the thumbs are made to press steadily on the one side of the lock, and the forefingers firmly to support the anterior shank on the other side. The point of contact is the fulcrum, against which each handle, as a lever, is made to press from opposite directions. Considerable force can thus be

applied, especially if the handles have transverse rests below the lock. Of course, the hands are ready to complete the locking as soon as the twisting or compression is sufficiently accomplished.

This paper might be considerably extended by considering what may be done in particular contingencies, but this is unnecessary. I wish my remarks to be taken as hints rather than elaborate details. I believe, indeed, it would be impossible to lay down rules that must apply to all the cases that one meets with in practice. A great deal must be left to the common sense of the practitioner in adapting general instructions to the peculiarities of individual cases. Where there is pelvic deformity, or mal-position of the head, or strongly developed pelvic ligaments, more depends on the mode of traction and general management than on the mere act of introducing the blades; but these are points on which I do not enter in the present communication.

Note on Modification of the Midwifery Forceps.—A few years ago I showed an instrument at this Society, intended to be such a modification of the long forceps, that the shank being cut at the middle, a joint was contrived by which the pieces could with facility be joined or detached. The Simpson forceps could thus be made as portable as the common short forceps. In contriving the joint, there was a rather intricate mechanism of spring and catch, to prevent the pieces becoming too readily disjoined. After some experience, however, I have discarded this mechanism as quite unnecessary, and for several years have found the simple instrument, which I now show to the Society, to answer admirably.



The two portions are joined by what is known as the bayonet joint. On the blade portion, *a*, there is a socket, in the rim of which is a notch, *b*, which leads to a horizontal slit extending about one-third round the circumference of the socket. On the handle portion, *c*, is a nipple

which fits into the socket, and on it there is a small knob which enters at the notch. When the nipple is fairly inserted, and a quarter turn is given to the handle, the knob slides along the horizontal slit, and the blade and handle portions have then exactly the same relations to one another that they have in the solid instrument.

In making the socket, it is very necessary to attend to the direction of the slit. In both blades the notch for receiving the knob of the nipple is opposite the concave *edge* of the blade, and the slit itself terminates opposite the concave *side*. It will then be found that, with either blade, the rotation necessary to join the pieces is in the same direction as the rotatory movement that must be given when the blade is to be adjusted to its position on the head.

On looking at the instrument as held in the hand, I dare say the first impression may be that the joints are insecure, as the tips of the blades appear to fall readily inwards. But it is to be remembered that in practical use the child's head is between the blades, and when they are locked the whole instrument is as immobile as the solid-stemmed long forceps. It is then an impossibility for either handle to slip away from its blade portion. The whole of the strain in compression and traction is thrown on the solid part of the socket.

In using the instrument, I first introduce and adjust the blades, and then join the handles. Sometimes, however, as when the head is at the brim of the pelvis, I may join the handle on the second or upper blade, to assist in the rotation necessary to place it in position on the head. I do not find that the blade, when once adjusted, tends to slip out of its place more readily than that of the ordinary instrument. After introducing the first blade, I have purposely left it alone for some minutes without finding it displaced. Indeed, one would naturally expect that the absence of the weight of the handle would be rather an advantage in this respect.

The act of joining the handles is a very simple process. The notch in the rim of the socket being felt for, the nipple is introduced with the knob opposite the notch. When fairly inserted, the handle of the lower blade is turned upwards, and that of the upper blade is turned downwards. The instrument now resembles the ordinary forceps, and locking is made in the usual way; of course, care is to be taken that the shank of the upper blade is in front of that of the lower when the handles are joined.

As, in my former paper, I narrated some cases to illustrate the advantages of this modification of the midwifery forceps, I shall not now detain the Society by further remarks. I may only mention that, after other nine years' practice, I have

never experienced any serious difficulty in regard to the speculative objections that were urged against the instrument. —*Edinburgh Medical Journal, August*, 1871, *p.* 117.

70.—ON USING LONG FORCEPS WITH THE PATIENT IN THE LATERAL AND SUPINE POSITIONS.

By Dr. ANDREW INGLIS, Professor of Midwifery in the University of Aberdeen.

In a former paper* I showed that, in short forceps cases by adopting the supine position and by introducing the blades from the front, as is done when using the catheter on a man who is in bed, the otherwise inevitable preliminary disturbance, and the consequent exposure of the patient are entirely avoided, while the instruments following during their introduction the antero-posterior curve of the pelvis, meet with the smallest possible resistance. As to extraction in that position of the patient, I showed that the operator had more power to act, and also that by using it, more relaxation of the soft parts at the outlet was obtained than by any other posture.

This position cannot, however, be adopted from the first in long forceps cases, for, when the instruments are properly adjusted on a head at the brim, the handles project backwards in the line of traction, and would be buried in the mattress were the patient lying supine in the middle of the bed, so that, to adopt the supine position at all in such cases we must bring the nates *over* the edge of the bed and double up the limbs as in lithotomy. This is the method universally adopted on the Continent but is not satisfactory. It is offensive, demands more assistance than is required in any other position, and, moreover, it puts the perineum more on the stretch than need be.

The plan I have adopted for some time for the high operation is as follows :—

The patient is laid on her left side diagonally in the bed, but not with the breech projecting over the edge, her head low, the limbs not drawn up much, only slightly flexed, the knees being kept a little apart by a pillow.

Introduction of Blades in Occipito-anterior Presentations.—The occiput having been ascertained to be near the right groin, the two first fingers of the left hand are introduced along the back wall of the vagina till the head is reached. Their points resting firmly on the head, are to be pushed up to the most posterior part of the presentation and retained there, keeping back the os and folds of mucous membrane. After this, the left half

* *Medical Press and Circular*, June 8th, 1870.

of the forceps is introduced, at first it is held horizontally, the handle projecting forwards between the limbs and the convexity of the blade lying in the palm of the examining hand. The blade is then made to slide in along the anterior surface of the fingers, observing the mesial line of the pelvis, till it arrives at the point where they touch the head. After this a partial rotation by pronating the right hand, is made in order to send the blade laterally, which results in a slight raising of the handle towards the patient's right hip. It is then pushed on, the handle passing in its progress backwards and downwards till it rests firmly against the perineum. The blade in its course through the vagina, traverses over the front of the pelvic portion of the rectum, and as it gets into the uterus, passes more forwards, and when finally adjusted, lies immediately in front of the transverse bi-section of the pelvis at the brim, with its tip resting on the head behind the left ear. The second blade is next introduced. The fingers of the left hand rest on the head as before, the right blade is passed along their palmar surface horizontally, keeping the mesial line of the pelvis till it reaches the head where the fingers touch. It is then rotated partially, by supination of the right hand, the handle is slightly depressed, and then pushed onwards, upwards and backwards, till the locks meet at the perineum. The second blade will lie in the posterior half of the right side of the pelvis at the brim, opposite the sacro-iliac joint, and should cover the right zigomatic region of the head.

In the second position of the head, the introduction is exactly similar in all respects, excepting that the blades must be made to grasp the head, while they lie in the reverse oblique diameter, the left blade being sacral, and the right pubic.

Traction.—When the patient is placed diagonally in the bed, with the breech far from the edge, the traction can be applied during the first part of the proceeding, in a most effective manner. Her body sinks into the mattress, and is not easily displaced, while the operator stands straight before his work in a good position for exerting force with accuracy. As the head progresses, the power of pulling and guiding gradually diminishes, for the handles get forward between the limbs; but this allows the patient to be turned on her back, and the case becoming a short-forceps one, is much more easily terminated in the supine position. Apart from theoretical considerations, proofs of this may be met with in practice—for I have more than once seen it happen, that where the head could not be moved while the patient was on her side, it passed through easily after she was turned on her back.

Occipito-posterior presentations.—With regard to the third and fourth presentations of the head at the brim, no such defi-

nite rules as the foregoing for using forceps can be laid down, for the course of the head through the pelvis in these presentations, varies considerably. For instance, it may turn as it advances into an occipito-anterior presentation, and may then be treated as such, or the occiput may descend into the hollow of the sacrum, so as to oblige the head to pass out of the pelvis with the face under the pubes. In the latter case the handles sometimes project so far backwards at the end of the extraction that it would be impossible to put the patient on her back in the bed, and the head may also emerge in the oblique diameter of the outlet, the occiput passing over the sciatic notch of one side. This oblique course of the head is by no means rare in such cases, and were the patient on her back, the handles could not be diverted sufficiently to one side.

The conclusions I would point at, in connection with this and the previous paper, are as follows:—

1st. In all long forceps cases, introduction of blades can be effected from the front.

2nd. Therefore, the patient can lie in the middle of the bed during introduction.

3rd. Consequently, traction during the first part of operation is more effective.

4th. After the head fills the pelvis, the case becomes a short forceps one (except in the rare cases quoted), which can be more advantageously treated by using the supine posture

Before leaving the subject, I would again refer to the pernicious custom of doubling up the patient, and thus tightening the soft parts concerned with the outlet of the pelvis; even in ordinary labour in primiparæ advantage ought to be taken of this supine position to slacken the perineum, and the relief which is given by doing so, is often very marked—most women, if left to choose their own position at the end of their first labour, turn on the back of their own accord, and if they do so, they can hardly be persuaded to turn again on the side.—*Medical Press and Circular, June* 21, 1871, *p*, 526.

71.—ON DECAPITATION AS A MODE OF DELIVERY IN SHOULDER PRESENTATION, IN WHICH VERSION CANNOT BE SAFELY EFFECTED.

By George H. Kidd, Esq., President of the Dublin Obstetrical Society.

[In cases of shoulder presentation in which turning is impracticable, or highly dangerous, owing to the shoulder being wedged in the pelvis, and the uterus firmly contracted upon the child, we have only the choice of two modes of delivery, evisceration and decapitation. The former is the most modern,

and, in some cases, the easiest, but more frequently it is difficult to perform, and dangerous to the patient. Decapitation is a much older operation; it was described by Celsus, and many writers since his day. Sir James Simpson considered it preferable to evisceration in cases of the class in question, as being safer to the patient and easier to the practitioner.]

The instruments that have been used for dividing the neck of the fœtus, in utero, may be arranged in three classes:—1st. We have hooks with variously devised cutting blades. The first of these, and the model on which the others of this class have been formed, is the decapitating hook of Ramsbotham, senior, which is, in fact, the ordinary blunt hook, with a cutting blade fixed in the concavity of the curve, and which resembles much the instrument described by Celsus. Davis, Scanzoni, Jacquemier, and others have modified this hook by the introduction of a guard or sheath, or of cutting blades, made to move by screws and levers in the handle, for dividing the neck. Dubois uses a pair of strong scissors with which he cuts through the neck, bit by bit, from below upwards. Professor Heyerdahl, of Bergin, adopted a different plan. In 1855, he invented an instrument consisting of a handle and hollow stem, together, 14 inches long. The extremity of the stem is curved and forms about a third of a circle. The instrument is hollow, and a strong wire passes up the centre. The wire is pushed up by means of a button sliding in the handle, and at its extremity there is a little knob which closes the open end of the stem. The instrument is used thus:—After it has been passed round the neck the internal wire is pushed upwards, and this projects the knob round and beyond the other side of the neck. A loop of string can now be passed round the knob, and, on withdrawing the instrument, a cord is thus left round the neck, with which a chain or wire rope is drawn round, with which the neck is sawn through, and, to avoid mutilation of the soft parts of the mother, the rope or chain is crossed in the operation.

Without appearing to have known anything of Heyerdahl's method, Pajot and Tarnier have adopted modifications of it. In the Archives Générales for September, 1865, Pajot published a paper on presentation of the shoulder, where there is extreme narrowing of the pelvis, and on a new method of embryotomy. This new method is virtually Heyerdahl's, and consists in sawing through the neck with a cord. To get this cord round the neck and, at the same time, to avoid the multiplication of instruments, he makes use of the handle of his forceps, which is very much bent, and the extremity of which he has perforated, and through this perforation he passes a cord, on the end of which is a leaden bullet. The cord is pulled till the bullet is closely applied to the end of the bent handle of the forceps,

which is then passed up into the uterus and hooked on the child's neck. The cord is now relaxed so as to allow the bullet to fall and roll down into the hand of the operator; but if the compression of the parts should prevent the ball from finding a passage, a simple pressure on the fœtus with a finger or the bent hook will immediately determine the formation of a sort of gutter in which the ball will at once engage itself. As soon as the cord is fixed in its place the instrument is withdrawn, and the ends of the cord are passed through a speculum, which is introduced into the vagina to save it from injury, and by drawing the cord rapidly backwards and forwards the neck is cut through in a few seconds. If the neck should be inaccessible, the cord may be placed round the body, between the crest of the ilium and shoulder blade, but it will occupy four or five minutes to effect the division here. Pajot's colleague, Tarnier, seeing Pajot's experiments, suggested the use of an instrument, such as is used for plugging the posterior nares (Balloc's Sound), for carrying the cord round the part of the child to be divided, which was the one feature wanting to make it virtually, and to all intents and purposes, Heyerdahl's operation.

During a visit to Paris, in November, 1869, I procured Pajot's instrument, and determined to give it a trial on the first suitable occasion, but it was not till the 8th January last that I had an opportunity of doing so. On that day I was asked by Dr. Harley, of Baggot-street, to see a poor woman, of whom he gave the following history. She had had six children, and always with difficult labours, owing to a narrowing of the brim of the pelvis. When he first saw her she had been three days in labour, and the hand and arm had been lying outside the vulva for 24 hours. He had passed his hand into the uterus, laid hold of a foot and brought it down into the vagina, but by no effort, that he thought justifiable, could he get the child to turn. When I saw her I found the foot in the vagina, where Dr. Harley had left it, and seized it, and tried to turn, but also found the uterus was so tightly moulded to the child that I could not complete the version; and as the fœtal heart could not be heard, I determined to divide the neck. I placed the handle of Pajot's forceps on the neck without very much trouble, but failed to make the bullet roll, and had to pass my hand behind the neck till I met the instrument, when I drew down the bullet, and so got the cord into its place. I now passed a speculum as directed by Pajot, and proceeded to cut through the neck, but at the end of one minute and a half of the sawing motion the cord broke, and I feared the operation had failed. On examination, however, I found the greater part of the neck, including the spinal column divided, and I now tried to bring down the foot and effect version, which I did

without any difficulty whatever, and the woman made a rapid and satisfactory recovery.

This is an example of the second class of cases in which version cannot be effected; those in which the hand can be introduced and the foot seized, but the body of the child cannot be turned. I think we may say these are the cases in which evisceration is most difficult and dangerous, and decapitation is most easily performed. Certainly, when I contrast the safety, ease, and rapidity with which the operation was performed, with the difficulty I have myself experienced and seen occur with several experienced operators in performing evisceration, I am most favourably impressed with the value of the operation of decapitation.

On reflecting on the case, it seems to me that the operation might be very much simplified and also rendered safer, and still without infringing Pajot's principle of not multiplying instruments. The cord might be very easily carried round the child's neck by means of an elastic catheter, mounted on a firm stylet, or, what would be better still, mounted on a uterine sound. The firmness and broad handle of the sound would enable us to direct more easily the point of the catheter. The catheter should be a good deal curved; it should form about the fourth of a circle, whose diameter would be three inches. The first step of the operation is to hook this on the child's neck; then the stylet being held steadily in its place, the catheter is to be run forward, when the curve of the stylet will cause it to pass round the neck till its point can be easily reached by the fingers, or may come into view. The cord may now be attached to the catheter, and drawn back with it, or it may have been previously passed through the catheter, and brought out at the eye of the instrument; so that it can be seized when the catheter comes within reach of the fingers. The ends of the cord are now drawn through a speculum, the catheter having been first removed, and the neck is divided as Pajot directs. If no speculum be at hand, the soft parts of the mother may, as Pajot suggests, be protected by the broad handles of two spoons introduced into the vagina, and used as retractors.

It will be seen that this suggestion differs from Heyerdahl's only in that the internal wire is held steady, and the outer tube is the part pushed forward. It is probable a soft elastic catheter would find its way more safely than the firm wire; but the chief object of the suggestion is that it does away with the necessity for a special instrument, which might not always be at hand when required.

The operation might be further improved by using an écraseur for dividing the neck, by which the accidental interruption of the operation by the breaking of the cord would be avoided,

as well as the risk of injuring the soft parts of the mother by its friction. Once the cord is placed round the neck there would be no difficulty in attaching the chain of an écraseur to it, and drawing it into its place. The use of an écraseur invented by Dr. Ritchie for this purpose was, according to Dr. Barnes, mentioned at a discussion at a late meeting of the Edinburgh Obstetrical Society by Dr. Keiller, but I have not been able to meet with a report of this discussion.

As soon as the neck has been divided, the body may be easily extracted by drawing down the arm, and if the uterus act well it will very probably expel the head; but if not, it may be seized with the forceps or cephalotribe. In using the cephalotribe in this case it will not be necessary to perforate in the first instance, because when pressure is applied the brain will escape through the foramen magnum; and, in avoiding the perforator, we avoid one great source of danger and difficulty in this part of the operation. But it is very probable that the best way of completing the operation would be to aim at the production of the condition that arose in the case under consideration from the accidental breaking of the cord—that is, to stop as soon as the spinal column is divided, and to leave the head attached to the body by a portion of the soft tissues of the neck; then to deliver by the foot, and let the head be drawn down still attached to the body. It is obvious that the division of the spinal column would do away with the difficulty of turning, and the tissues attached to the head would afford us a ready means for its extraction.—*Dublin Quarterly Journal, May*, 1871, *p.* 383.

72.—TREATMENT OF HEMORRHAGE ARISING FROM RETENTION OF THE SECUNDINES AFTER ABORTION.

By Dr. J. G. Swayne, Physician-Accoucheur to the Bristol General Infirmary.

An abortion about the middle of utero-gestation may become one of the most troublesome, difficult, and dangerous cases which can fall to the lot of an accoucheur. The reason is, that at this time it is peculiarly liable to be complicated with retention of the secundines. During the first two months of pregnancy, the aborted ovum is usually cast off entire, and there is seldom any further trouble. During the last three or four months, the membranes generally are ruptured, and the fœtus and placenta expelled separately; but the latter is sufficiently bulky to stimulate the uterus to expel it; and, even if this should not happen, the uterus is large enough to admit of the introduction of the hand for the purpose of removing it. During

the middle of pregnancy, however, or rather, I should say, during the third, fourth, and fifth months, the ovum is very likely not to be expelled entire: the placenta and membranes are not sufficiently bulky to induce expulsive efforts of the uterus, and are, therefore, very liable to be left behind; and the uterus is not large enough to admit the hand for the purpose of extracting them. Under favourable circumstances, the placenta and membranes may be cast off with, or immediately after, the fœtus; but it is quite as usual, I think, to find that this is not the case, and that the secundines, if let alone, may be retained for an indefinite period. For instance, on making an examination, the accoucheur finds the uterus contracted in all its dimensions, and the os uteri tightly encircling the filiform funis, which will inevitably give way if any attempts are made to remove the placenta by its means. In fact, circular contraction of the fibres of the os uteri internum, or what is commonly called hour-glass contraction of the uterus, is a much more frequent occurrence after abortion than after labour at the full term, and is accompanied with similar dangers, although in a minor degree. As long as the placenta and membranes remain behind in the uterus, the patient is subject to attacks of hemorrhage, which may be very frequent and very profuse; or she is liable at a later period to septicæmia from the absorption of putrid matter caused by the decomposition of the retained secundines.

To prevent such untoward results, the obvious indication is, of course, to remove the exciting cause; but this is a matter of so much difficulty in practice, that obstetric authorities have for a long time been divided in opinion as to the advisability of attempting it. The older authorities—for instance, Denman, Davis, Dewees, Burns, Ingleby, and Blundell—are in favour of the expectant plan, and disapprove of manual attempts to extract the placenta. Denman, who always relied so implicitly on the *vis medicatrix naturæ*, is especially opposed to manual interference, and at the same time very much underrates the risk arising from retention of the secundines, for he goes so far as to say, "Much less mischief may be expected from the retention of a putrid placenta than from attempts to force it away by the medicines usually given, or by manual assistance." I have myself in many instances tried the expectant plan of treatment, and can by no means give a favourable report of its results. As long as the placenta remains in the uterus, the patient is liable to sudden and alarming attacks of hemorrhage; and so greatly am I impressed with the danger of this occurrence, that during the first three or four days after an abortion I would not on any account leave a patient with the placenta in the uterus, unless I had previously guarded against this acci-

dent by plugging the vagina. In all cases of miscarriage accompanied with much hemorrhage, the plug is a most valuable remedy, and may always be employed when the placenta cannot be extracted. It is a very safe remedy also, when certain precautions are taken. These are, to watch carefully that it does not occasion accumulations of blood, or retention of putrid matter, in the uterus. To fulfil the first indication, firm counterpressure should be made over the fundus uteri, so as to prevent the expansion of that organ. This is unnecessary during the first three months of pregnancy, as the uterus is too small to hold much blood; but during the fourth, and especially the fifth month, it becomes necessary, because the uterus is then capable of containing a considerable amount. I once attended a woman who miscarried during the fifth month, and who nearly died from internal hemorrhage into the uterus, although the vagina had been plugged, and there was not the slightest escape of blood externally. She had lost a large amount of blood previously, and the quantity which then escaped into the uterus was nearly enough to turn the scale against her. Her case so impressed the danger of internal hemorrhage on my mind, that I now never use the plug in the fourth or fifth months of pregnancy without applying over the fundus uteri a large cup-shaped piece of padded metal made for that purpose, and just adapted to the size of the organ. This is secured in its place by a strap and buckles, and over all an abdominal bandage. To fulfil the second indication—viz., to obviate the danger of collections of putrid matter in the uterus —the plug should be removed frequently, and reapplied if necessary. As a general rule, it is well not to let a plug remain *in situ* more than twelve hours. By far the most efficient method of plugging is to introduce into the os uteri a tent of compressed sponge before plugging the vagina. This not only prevents all escape of blood from the uterus, but most effectually dilates the cervix, and facilitates the extrusion of the placenta. The vagina should then be plugged; and for this purpose I generally prefer an old silk handkerchief, which has been previously torn into two or three long strips, and well oiled. It is much more convenient to introduce the plug through a full-sized tubular speculum. When the plug is pushed up by the fingers without using a speculum, the operation is much more tedious and painful. The plug should be removed after an interval of about twelve hours: and it is a good plan to administer a full dose of ergot about two hours previously. The result of this will probably be that, on removing the plug, the retained placenta will come away also; the stimulus of the plug and the ergot having induced expulsive action of the uterus. But yet, in many other cases, the action of ergot is

very uncertain, and one's hopes are doomed to disappointment. If the placenta does not come away, plugging may be continued for two or three days, if there be much danger of hemorrhage, but if this is not the case, it is better not to persevere with it so long. When putrefaction of the secundines has come on, and the vaginal discharge has become very offensive, the plug should be taken away: and this is above all necessary if fever and constitutional irritation have set in. The plug, by confining putrid discharges within the uterus, renders the danger of absorption more imminent. To obviate this danger the vagina and even the uterus should be syringed out twice or three times a-day with warm water and antiseptic lotions, and this should be done quite as much with the view of detaching the placenta as of neutralising and washing away the putrid discharges. An ordinary Higginson's syringe with an elastic tube is the best instrument for washing out the vagina, and the nurse or other attendant may be directed to throw up about a pint of fluid each time. To syringe out the uterus, however, requires much more care and caution, as it is a proceeding by no means free from risk. I have seen very alarming symptoms, such as sudden acute abdominal pain, dyspnœa, and great prostration produced by injections into the uterus, especially when the fluid has been thrown up too forcibly and in too great quantity; and there is no doubt that in this way, not only some of the injection, but even some of the putrid uterine contents may be forced through the tubes into the abdominal cavity. On this account no more fluid should be injected than the uterus can easily contain; and the tube used for the purpose should not be so large as to prevent a free escape of the fluid by the side of it through the os uteri as soon as the uterine cavity is filled.

A small India-rubber bottle, to which an elastic catheter has been adapted, will be much better for uterine injection than a Higginson's syringe. I need scarcely say that this operation should not be entrusted to a nurse. Besides antiseptic lotions, astringents and styptics may be injected in the manner just described whenever hemorrhage comes on after plugging has been discontinued. For this purpose an injection consisting of four ounces of the liq. ferri perchlorid. fortior of the *British Pharmacopœia* with twelve ounces of water, as recommended by Dr. Barnes, will be found very serviceable. I am in the habit of using a lotion consisting of one part of Bird's styptic (a peroxide of sulphate of iron and alumina) with four parts of water, and I prefer it to any other. Besides these local means for checking hemorrhage cold may be applied, and ergot of rye administered, together with various astringents. The recumbent posture should be strictly enjoined.

It must be confessed, however, by all who have had much experience of these cases, that the expectant treatment is very unsatisfactory; and that, in spite of plugging, styptics, and other palliatives, the patient is never safe from hemorrhage. septicæmia, and other less dangerous consequences, so long as the *fons et origo mali* in the shape of the whole or any portion of the secundines is left behind in the uterus. She may suffer for weeks from severe hemorrhages, frequent rigors, constant fœtid vaginal discharges, hectic fever, night sweats, great prostration of strength, tenderness and swelling of the abdomen, and complete loss of appetite; and these symptoms may go on to a fatal termination unless the putrefying portions of retained placenta are either expelled naturally or removed by art. On this account most obstetric authorities in the present day have abandoned the expectant plan, and are in favour of active interference.

In 1861, Mr. Priestley communicated to the Obstetrical Society of London an excellent paper on "the Treatment of Cases of Abortion in which the Placenta and Membranes are retained." In the discussion which followed, the weight of opinion in the society was decidedly in favour of manual interference: Dr. Priestley, Dr. Hall Davis, Dr. Tyler Smith, and Dr. Tanner, all strongly advocating this plan of treatment, in which opinion I myself fully concur.

The propriety of manual interference being decided in the affirmative, the next question will be as to the best method of effecting it. Most authors maintain that the safest means is by the hand alone, and condemn (without, as I think, sufficient reason) the use of any kind of instrument for this purpose. As I remarked before, the uterus at this period of gestation will not admit the entire hand, and the most that we can do is to introduce the hand into the vagina so as to pass the fore and middle fingers into the uterus. Any attempts to extract a retained placenta, by two fingers, in the vagina only, will almost certainly be futile. Under the most favourable circumstances, the tips of the fingers may thus be able to touch the placenta just within the os uteri, but have little or no power to bring it down, and, in all probability, it will again and again elude their grasp. It becomes necessary, then, to introduce the whole hand into the vagina. But this is always a very painful operation at this period, even in a multipara, and in a primipara is accompanied also with considerable difficulty, and not free from danger. It is, therefore, always best to place the patient first under the influence of chloroform, as much for the purpose of relaxing the tissues as of abolishing pain. The *modus operandi* is thus described by Dr. Priestley: "Whilst the woman lies on her back, with the thighs flexed upon the abdo-

men, I have introduced one hand into the vagina, the other hand being placed over the fundus uteri, externally, to steady and depress it. If the os uteri was closed, it was gradually dilated by passing first the forefinger into the orifice, and then the second, with as little force as possible. With time and caution, the os uteri gives way to gentle pressure, and the introduction of the finger is in most cases not difficult. When the placenta is reached, it is separated in the ordinary way, and pushed downwards towards the palm of the hand, until it is partially or wholly beyond the os uteri. It is important, certainly, to ascertain that the placenta and membranes are entirely loosened from their attachments, and quite movable in the uterine cavity before the retraction of the hand, so that a second introduction of the fingers is not needed." Dr. Priestley further remarks: "In scarcely any case can it be necessary to introduce more of the hand into the uterus than the first two fingers—one may occasionally answer the purpose, but it works at great disadvantage alone, both in separating and extracting the placenta. The two fingers together constitute the best form of forceps for seizing what it is intended to withdraw, and are the most serviceable in all respects." With respect to the time that ought to elapse after the birth of the fœtus, before the placenta should be extracted, Dr. Priestley is inclined to suggest that six hours may be fixed as an approximative limit, unless hemorrhage have occurred to any extent during the interval. In such a case their removal should be effected earlier.

As far as my own experience goes, I cannot bear out the statements which have been made as to the efficiency of two fingers, either in dilating the os uteri, or extracting the placenta and membranes; I have several times failed, especially when the placenta has been at all adherent, in effecting these objects. This I attribute to the small powers of abduction and adduction which the fingers possess, even under ordinary circumstances. It is surprising how little power there is of grasping anything between the distal phalanges of the two first fingers, compared to that which is possessed by the forefinger and thumb. To test the relative powers of each, I have tried what amount of weight I could raise by the forefinger and thumb only, used as a forceps, and by the two first fingers. I found that, while I could raise twenty-eight pounds by the former, I could not raise more than four pounds by the latter. The reason, no doubt, for this great difference in power is, that when anything is grasped between the points of the thumb and forefinger, it is held by means of the flexor muscles; whereas, when it is grasped between the points of the fore and middle finger, it is held solely by the adductor muscles, which

are much weaker and act much less advantageously. To show how little power the two first fingers have of grasping anything between them, let any one place the handle of an ordinary dining-room poker between their two distal phalanges, and he will find it no easy matter to lift it off the ground. One can readily understand that these two fingers, enclosed in a contracting uterus, would form but a very inefficient forceps.

On this account I have endeavoured to supply the deficient powers of abduction and adduction possessed by the fingers by means of a modification of the ordinary forceps which is used for extraction of the ovum. The extremities of the instrument form an obtuse angle, with the shanks so as to correspond with the angle formed by the axis of the uterus with that of the vagina. The extremites are fenestrated, and resemble in shape the blades of an ordinary obstetric forceps in miniature, except that they are more hollowed on their inner surface, so as to fit with tolerable accuracy over the forefinger of the left hand when it is placed between them for the purpose of introduction. Attached to the handles is a screw, by means of which they can be powerfully separated when necessary. The instrument may thus be made a most efficient agent in dilating the os uteri.

The cases most adapted for the use of this forceps are those in the os uteri which will just admit the last phalanx of the index finger, and where the detracted placenta can just be felt at the os uteri internum, but is found to be retained in that position by the contraction of the circular fibres of the os uteri. The forefinger of the left hand enclosed between the blades of the forceps, but with the tip advanced a little beyond them, should be passed through the os uteri until it touches the placenta. The blades of the instrument should then be passed in a little beyond the finger, and separated very slowly by the screw, until the contraction of the circular fibres is gradually overcome. The placenta will then probably be extended until it can be grasped by the blades of the forceps, and if the cord is unbroken, this process may be facilitated by a little gentle traction upon it. In order to grasp the placenta, the screw must be turned in a reverse direction until the handles can be brought together.

Much has been said about the danger of passing instruments of this kind into the uterus beyond the guidance and protection of the fingers. I can readily understand that, with such an instrument as the wire crotchet of Dr. Dewees it would not be difficult to injure the inner surface of the uterus when groping about for a retained placenta; but I cannot see how it would be possible to grasp any portion of the uterus with

such a forceps as I have described when used with ordinary care; and the blades are so smooth and rounded externally, that they would not be likely to inflict injury in any other way. For my own part, I should not hesitate to employ this forceps, even when the placenta is still adherent to the fundus uteri. The advantages it possesses are that it dilates the os, and grasps the secúndines better than the fingers, and that it renders the introduction of the entire hand into the vagina unnecessary.—*British Medical Journal, August* 19, 1871, *p.* 201.

73.—ON A CASE OF PUERPERAL CONVULSIONS TREATED BY CHLOROFORM AND VERSION.

By Dr. J. Harris Ross.

I was sent for on Sunday, Dec. 5th, 1869, at 6 p.m., to see Mrs. C., aged 22, who had fallen from her chair in a fit. She was lying on her bed, and had a flushed face and bounding pulse; and expected daily to be delivered of her first child. I was informed that the fit was sudden, and came on whilst sitting at tea; that she struck her head against the table in falling; that she had complained of headache all day, but had been about her household duties. She had most violent paroxysms of convulsions, which came on about every ten minutes, leaving her during the intervals perfectly unconscious.

On making a vaginal examination, I found the os extremely high up, so as hardly to be reached with the finger; it was not at all dilated, though very flaccid and abounding with secretion. There was no "show." I at once sent for a medical friend of mine, and, after consultation, we decided to give chloroform, and try to evacuate the uterus as quickly as we could. After placing a cork between her teeth, my friend gave her chloroform, whilst I proceeded to dilate the uterus with my fingers. After gradual extension of the os for about half an hour, I was enabled to introduce my left hand into the uterus, and had no difficulty in turning, and bringing the legs beyond the vaginal opening. My friend then took my place, and speedily completed the delivery. After the removal of the placenta there was a considerable escape of blood; but this was easily controlled by external manipulation over the uterus. After a little artificial respiration, I was pleased to find the child was alive.

Our patient had but one attack of convulsions after the administration of the anæsthetic, and that was during a slight interval when we had removed the lint from her face. Half an hour after delivery, her pulse had diminished from 140 to 120, and she had no convulsions. We then left the house, and

on again visiting her an hour afterwards she had had no return of the convulsions, but was still unconscious.

Dec. 6th. Seen three times during the day, in a semi-conscious condition, and when spoken to sharply replied, "Don't;" "Let me alone." Has neither eaten nor drunk anything since delivery; urine dribbles away involuntarily; has one hand constantly between her legs; pulse 126. She is very morose when offered nourishment, and on one occasion knocked a cup out of my hand.

7th. Took a cup of beef-tea freely during the night; cerebral symptoms and moroseness much the same as yesterday. I ordered clean linen to be appled, and the administration of beef-tea, eggs, milk, and brandy, &c. I saw her again in the afternoon, and found that none of my directions had been carried out, and that the nurses were both in a state of intoxication. I at once had the services of these dipsomaniacs supplanted by that of a temperate woman. In the evening I found the patient had taken nourishment liberally, had passed water at will in a vessel, and would answer a question when asked; still she was not quite conscious.

8th. Asked questions of the attendant during the night, who showed her the baby, and the patient laughed at the idea of its being her own. Talked quite rationally, and ate a chop for dinner.

From this date she gradually recovered her strength. She sat up for two hours on Dec. 15th, and was discharged quite well on Dec. 27th. She states that she remembers nothing from the morning of the 5th (when she had a severe headache) until that of the 8th. I am sorry I did not have an opportunity of examining the urine during the first two or three days, for I was about introducing the catheter on the occasion of her being first placed under chloroform, when the sphincter gave way and all the urine escaped, and for the next two or three days it kept dribbling away. She had no sign of lactation. On Jan. 16th, 1870, I met her in the street, carrying her baby. They both looked hearty and well.

This case is interesting at the present time owing to the diversity of opinion which exists as to whether we ought to bleed in these cases. On the one hand, we find Drs. Churchill, Johnson and Sinclair, Ramsbotham, Tyler Smith, and others, all strongly recommending bleeding. In fact one of them says: "Bleeding is our great reliance; the lancet is our sheet anchor; and blood may be taken to a large extent." On the other hand, we have the late Sir J. Simpson, Dr. Braun of Vienna, Dr. Barnes, Dr. Tanner, &c., all equally averse to bleeding. Dr. Tanner, in his last work on "Diseases of Pregnancy," says: "I have no hesitation in expressing a decided

belief that bloodletting, as a general rule, without exerting any valuable effect on the symptoms, is calculated to produce irreparable mischief."

Since writing the above I have read in the last volume of "Guy's Hospital Reports" a paper by Dr. Phillips, in which he relates an interesting series of cases all treated without bleeding.—*Lancet, August* 12, 1871, *p.* 215.

74.—TWO CASES OF PUERPERAL CONVULSIONS, TREATED BY MANUAL DILATATION OF THE OS UTERI AND INSTRUMENTAL DELIVERY.

By Dr. E. HOLLAND, Regent's Park, London.

In the course of the year 1869 it was my professional fortune to be called upon to treat two cases of severe puerperal convulsions in the duration of one week, and as there were certain peculiarities attached to each, some novelty in the treatment adopted, and in each instance a good recovery, I regard their brief announcement to be as desirable as it must be interesting.

Case 1.—A tall, full-fleshed, rather ruddy, and tolerably vigorous rural primiparia, the subject of aortic regurgitation and some hypertrophy of the left ventricle, with habitual constipation and albumen in the urine, was suddenly seized with epileptic convulsions, about the middle of the seventh month of her pregnancy, after a few hours' premonition of hazy vision, headache, and incipient labour pains. The convulsions were very severe, recurred at intervals of ten, fifteen, or twenty minutes, and were followed by insensibility and coma. The rectum was full, the os uteri massively thick, and open to the extent of a shilling, but the presentation could not be decided upon. The pulse was enduringly quickened, and forcible. An enema of gruel, containing an ounce of castor-oil and a flavouring of turpentine, was prepared with despatch, and administered after about a pint of blood had been drawn from the arm; the bladder was emptied by a catheter, the hot head had cold water applied to it, and the legs were wrapped in turpentine stupes. After the bleeding there was no fit for twenty-five minutes, and during this interval a piece of deal was placed between the teeth. The fits now recurred every ten or fifteen minutes with the pains, and were equally severe and prolonged, whilst the intermediate coma, if anything, deepened, and left scarcely a shadow of hope that she would recover. An anxious hour or so still passed, during which the convulsions were decidedly severer and more prolonged, and something was to be done. I now determined to introduce my hand (a small one) into the vagina, and to dilate the os uteri (now but little

altered) by introducing finger after finger, until I could work in the hand, turn, and extract. After about an hour's manipulation of the os, I succeeded in getting in the hand, and no sooner was it there than it came upon the fœtal feet, which were seized, and the extraction cautiously proceeded with, both during the pains and their intervals. As the shoulders came along, much caution was used to sweep the arms from the grasp of the uterus, and when all but the head was released from the uterus the latter became forcibly contracted upon it and round the fœtal neck. As the convulsions recurred, the uterine action was more powerful than I ever experienced; however, in the intervals of the seizures I contrived to insinuate my fingers and perforate the head behind the ear, and then, by very protracted efforts, effected its delivery by the conjoint use of the blunt hook and the fingers of the right hand. As soon as the child was delivered the convulsions ceased, and never returned. A comatose state continued for some ten or twelve hours, but yielded to a large blister on the nape; and, excepting hazy vision and occasional headache, she made an uninterrupted and speedy recovery, with no indication of uterine injury, and, quickly becoming pregnant again, miscarried about the eighth month, without convulsions.

Case 2.—A short, slim, and pale multipara, aged 42, with mitral reflux and albuminous urine, was suddenly seized, about the seventh month of pregnancy, with epileptic convulsions, after having been reduced to a state of incipient collapse by a severe attack of diarrhœa. When visited, the head was cool and perspiring, the legs and feet cold, and the pulse quick and feeble. There was a convulsive seizure about every ten or fifteen minutes, and a semi-conscious state in the intervals. A vaginal examination revealed the fact that labour had commenced. Altogether the case was as disheartening as any case could well be; but, fortified by my recent experience of the preceding case, I ordered hot bottles to the feet, turpentine stupes to both legs, and brandy-and-egg mixture to be cautiously smeared over the interior of the mouth. I watched an hour or so, and matters got worse. I then resolved to dilate the os uteri, as in the preceding case, and deliver by perforation or otherwise as quickly as possible. After an hour's digital manœuvring, I effected an entrance of the hand, and found a head presenting. I then immediately perforated; but after the escape of the brain, I had to remove the scull-cap piecemeal before I could get a firm hold. At last, however, after many a slip, I got a purchase on something, which turned out to be the clavicle, and by it I extracted a small female child of seven or eight months, drawing the uterus down, however, to the very outlet, where its retirement over the fœtus could be

seen. The placenta followed; but, to mend matters, I now found there was a second fœtus in the uterus. However, as there had been no fit for a good twenty minutes, and no additional complication, I let well alone (having always derived advantage from that maxim), and watched. Presently there was a strong pain or two, and the second child, enveloped in its membranes, and with its placenta, was expelled *en masse*. The patient had no more fits, took an opiate, and made a very rapid and complete recovery.

Both these cases were severe. Both were treated on general principles as far as the bodily health was concerned. In both the fits recurred with increasing violence, and their intermediate coma deepened, despite every general measure. In both cases the os uteri was dilated from a very small size by the introduction of finger after finger; and in both instruments were freely used during an operation of several hours. In neither case was there any consideration to be given to the child, it being premature, and well known in the majority of such cases to be born dead. In neither case was there any fit after complete delivery of the child, or any indication of uterine injury; but, on the contrary, a rapid and good recovery. And, as each case formed the opposite end of an associated series, they will bear to be reflected upon, notwithstanding the sweeping condemnation some might feel disposed to inflict on every procedure that deviates from the groove of *ex cathedrâ* practice.—*Lancet, Sept.* 2, 1871, *p.* 322.

75.—ON THE MODE OF EXAMINING THE UTERUS IN CASES OF SUSPECTED DISEASE.

By Dr. LOMBE ATTHILL, Fellow and Examiner in Midwifery King and Queen's College of Physicians; and Obstetric Physician to the Adelaide Hospital, Dublin.

[Nothing is more important than care and skill in making an examination of the uterus. The trouble involved, and the natural repugnance of the patient, too frequently lead to incomplete, and consequently, misguiding examinations, resulting in loss of credit to the practitioner.]

In examining nearly every case of uterine or vaginal disease, we require the aid of both touch and light, to arrive at a correct conclusion as to the condition of the affected organs. To use the speculum without a previous examination by the finger and hand, is not only wrong, but fails to convey to us anything like an accurate knowledge of the case. Thus a patient suffers from leucorrhœa with pelvic pain and pains in the thigh. You make an examination with the speculum, and

find the os uteri healthy, and may hastily come to the conclusion that no abnormal condition of the genital organs exist; and perhaps assure the patient that the womb is healthy. But nevertheless she is dissatisfied, for her sufferings continue, and by and by she consults another practitioner who detects the existence of a retroflected or anteflected uterus—a condition which an ocular inspection of the os uteri failed to detect. I could easily multiply examples, but let this one suffice to impress on you the necessity of making a manual examination before using the speculum. Now in speaking of a manual examination I mean more than a digital examination of the vagina. I include also under that term the investigation of the pelvic viscera through the abdominal walls, and if the symptoms seem to demand it, through the rectum also. I shall make a few remarks on the mode of conducting these investigations. First then as to the ordinary digital examination of the vagina or uterus. The patient is placed on her left side, the knees should be well drawn up, and the hips pushed out to the edge of the couch. These preliminaries effected, the index finger previously well oiled should be introduced slowly upward in the axis of the outlet of the pelvis; the tip of the finger being kept in contact with the posterior wall of the vagina. By adopting this course the finger reaches the posterior *cul de sac* of the vagina and by carrying it from this point round the cervix uteri, we are enabled at once to ascertain the condition of the lower segment of the uterus. Thus we learn whether it be moveable or fixed; whether it be of the normal size and shape; or if on the other hand elongated or hypertrophied. Then by drawing the finger down along its surface you reach the os uteri and discover its state, whether it be patulous with everted lips, or small and contracted. While thus engaged in investigating the condition of the uterus, you should not fail to attend to that of the vagina, and to satisfy yourself whether it be of the natural temperature and moisture, or unduly hot and dry. But there is more yet to be ascertained before you have gained all the information possible from a digital examination —the position of the uterus itself is to be made out, for the organ may be retroflected or anteflected, or possibly under certain circumstances completely retroverted.

As a rule you should not be able to feel the body of the unimpregnated uterus through the posterior *cul de sac* of the vagina. If therefore on sweeping the finger round the cervix you feel a firm globular mass above you, you can at once pronounce that the organ is in an abnormal condition. Then immediately follows the question, which you are called upon to solve, namely, on what does this enlargement depend? But I must defer the consideration of this question to a future lecture, for a mere

digital examination though of importance, is frequently insufficient to enable us to decide this point; and in a large number of cases you must not remain content with it, or you will fall into grave errors. To make your examination complete you must have recourse to the use both of the speculum and of the uterine sound. I name them in the order in which as a rule they should be used. You see on the table there are three kinds of speculum; they are all of them admirable instruments, and, as I am about to explain to you, each possesses certain advantages which the other wants, and certain disadvantages which renders the use sometimes of one and sometimes of another preferable. It is, therefore, essential that you should be acquainted with the respective merits of each. There are no doubt numerous other kinds, but for ordinary purposes these are sufficient, and of these for general use I without hesitation recommend the one known as Fergusson's. It is as you are aware, a glass cylinder silvered externally. This again is protected by a layer of gutta-percha, which answers the double purpose of affording a very smooth surface, and serving as a protection to the vagina, should the glass by any mischance crack or break. Through a full-sized one of these speculums you can see the parts very distinctly, and it also possesses this great advantage, that it is uninjured by the action of acid, a class of remedial agents which are frequently used in the treatment of uterine disease. It is not however so easily introduced as either of the other speculums which I exhibit. If therefore the vagina be narrow or if much inflammation be present, the attempt to use a full-sized one will give so much pain that you will have to desist, and should you with the view of avoiding this, have recourse to a smaller one, you will find much difficulty in bringing the os into view; and even when you succeed in doing so, the position of the cervix exposed to view will be of such limited extent as often to afford but little information. Still the number of cases to which it is inapplicable will prove to be comparatively few. When from the narrowness of the orifice of the vagina, or from the amount of inflammation present you find Fergusson's speculum to be unsuitable, I recommend you to make use of a plated bi-valve. It is very easily introduced, but it does not reflect the light nearly so well as the glass one does, and moreover the lateral folds of the vagina fall to a considerable degree into the space between the blades when they are expanded, and intercept your view. The duck-bill speculum affords you one advantage which neither of the other possesses — namely, that it permits you to see the os uteri, and at the same time to touch it, a matter of the greatest importance in many cases. We therefore

use it when introducing sea-tangle or sponge tents into the cervix uteri or when having withdrawn these we proceed to examine the condition of, or to make applications to, the canal of the cervix or body of the uterus, and also in the case of all operations about the vagina or uterus. Its disadvantages are that the forcible drawing back of the perineum, which is necessary to permit the os uteri to be seen, causes pain; while if the instrument be not held very steady the os slips out of view. Next, that an assistant is absolutely necessary to take charge of it, and thirdly that difficulty is often experienced in keeping the anterior wall of the vagina from intercepting the view unless, indeed you seize the os with a hook or vulsellum—the reason for, and the mode of doing which, I shall on a future occasion explain. I shall now give you a few directions as to the mode of introducing the speculum, for if you use the instrument in a bungling unhandy way not only will you give your patient much unnecessary pain, but you will also most likely leave an unfavourable impression as to your skill on her mind, and I therefore feel that I am not wasting time in dwelling on these minutiæ. First then you should dip your speculum into warm water to bring it up to the temperature of the body, and oil it; then, your patient lying on the left side with the hips well out, you should with the index and middle finger of the left hand raise and draw up the right labium and nympha, while with the thumb and index finger of the right hand you hold the speculum, and bring its point to the orifice of the vagina. You should at the same time, with the middle finger of that hand, depress the soft parts on the left side; for if this be not done, and if the labia or nymphæ be turned in before the edge of the speculum you will cause your patient much unnecessary pain, which a little care on your part would have obviated.

When once the point of the speculum has fairly entered within the vagina, its further introduction is a matter of no difficulty; but still it is very possible for a person inexperienced in its use to fail in bringing the os uteri into view, and therefore you should be careful to keep the point of the instrument pressed well back against the posterior wall of the vagina, for the os uteri should look downward and backward, and by keeping the point of the instrument in the direction I have indicated the os should without difficulty come into view. If this be not the case, the speculum should be withdrawn a little way and its direction slightly altered, when the desired object will most likely be attained. The foregoing directions hold equally good, whether you use Ferguson's or the expanding speculum, for though the latter, on account of its shape, is

introduced with greater facility, yet it is not easier with it to bring the os into view; indeed the reverse is the case.

The duck-bill speculum requires special directions for its use. The following are those given by the inventor, Dr. Marion Sims, and should be carefully attended to whenever this speculum is used—"The thighs are flexed at right angles with the pelvis, the patient lying in a semi-prone position on her left side, her left hand being drawn backwards under her, and kept in that position; the chest rotated forward, bringing the sternum very nearly in contact with the table or couch, the head resting on the parietal bone; the head must not be flexed on the sternum nor the right shoulder elevated; the patient is thus rolled over on the front, making it a left lateral semi-prone position. The nurse or assistant at her back pulls up the right side of the nates with the left hand, while the surgeon introduces the speculum, elevates the perineum, and gives the instrument into the hand of the assistant, who holds it firmly in the desired position." These directions are admirable, and should be strictly attended to.

When with either speculum you have exposed the os uteri, you are able to judge of its state. You see first of all what may be the condition of the lips; if they are covered with healthy mucous membrane, and present the normal light mother-o'-pearl coloured appearance, or whether they be congested, abrased, or in a state of granular ulceration and bleeding on the slightest touch; you see also whether the os be a small opening, free from discharge, or whether it be patulous, and plugged with a string of thick, glairy mucus, the sure indication of an unhealthy condition of the cervical canal. Then while withdrawing your speculum, you have an opportunity of satisfying yourself as to the condition of the vaginal mucous membrane; thus by touch and sight you are enabled to pronounce with positive certainty as to the state of the os, of the lower segment of the cervix uteri, and of the vagina; but should you stop here, you will in many cases have failed in your duty. Many a sufferer has been told after having submitted to such an examination that the womb was perfectly healthy, because the os and cervix appeared to be free from disease, and has consequently been looked upon as a complaining hypochondriac by her friends; while in reality she was a suffering invalid—the physician having failed to detect the actual ailment, either because he omitted to carry his investigation further, or because he was ignorant how to do so. For myself I lay down the following rule, which I advise you to follow, in the investigation of all cases of uterine disease which come under your observation:—1st. To make a digital examination of the vagina and cervix uteri; 2nd. If that fails to satisfy me as

to the cause of the patient's suffering, then to use the speculum; and 3rd. If still in doubt, to introduce the uterine sound, unless its use be clearly contra-indicated. You are aware that the sound is an instrument of recent invention; but even so it is surprising how little it is used, and how few appreciate its merits. I look on it as at once one of the most useful and at the same time, if carefully and judiciously handled, safest of obstetric instruments. In my own practice I am indebted to it for most important information, which could have been obtained by no other means, and this too without having ever known it to produce the most trifling injury. Doubtless I am aware that if roughly and unskilfully handled, or used in an improper case, the most serious consequences may follow its introduction; but the same may be said of the catheter, or indeed of any other instrument requiring skill in its use. I again repeat that, if carefully used and skilfully handled, it is a harmless instrument, and may be used as safely or as freely as a catheter. Before explaining to you the mode of introducing it, I wish to call your attention to the instrument itself. It is as you see a metallic staff, not unlike the sound used by surgeons in the male. The best are made of copper plated, the advantage of which is that you are able to bend it at pleasure, a matter of no small importance, as you are frequently obliged to alter the curve when flexions of the uterus exist. At a distance of two and a-quarter inches from the extremity of the instrument there is a little knob, which marks the depth to which it should usually penetrate into the uterine cavity; and at this point you observe the instrument is curved, so that it may correspond with the axis of the uterine cavity. The entire length of the instrument is marked at intervals of an inch by notches, which enable you at once to decide to what depth the instrument has penetrated, for when withdrawing it you keep the point of your finger on the notch nearest to the os, and with the aid of the figures marked on the handle see at a glance what the depth of the uterine cavity is. It is not a matter of any great difficulty to introduce the sound into the cavity of the uterus; still it requires tact and practice, just as the use of the catheter does. The following directions will aid you in acquiring the requisite skill. I recommend you to introduce the index finger of the right hand into the vagina, and to keep the tip in close contact with the os uteri, then to guide the point of the sound held in the left hand up to the os, slipping it along the inner surface of this finger, the concavity of the instrument being turned towards the rectum. A little manipulation and gentle pressure will now make it enter the canal of the cervix. This being fairly accomplished, a fact you can always be sure of because your finger is still in contact

with the os, you are to rotate the handle of the sound, a manœuvre exactly similar to that practised by surgeons when introducing the catheter in the male, and termed "tour de maitre." This has the effect of changing the direction of the point of the instrument, which will now look upwards in the direction of the axis of the uterus; steady but very gentle pressure should now be made, and the point will in general pass on without difficulty till it reach the os internum, and here some slight difficulty is generally met with. This if it occurs should be overcome by gentle continuous pressure; force must not on any account be used, lest injury be done to the uterine walls. As the point of the instrument passes through the os internum, the patient nearly always complains of pain and sometimes of nausea; but this goes off in a few minutes, though I have met with instances of the pain lasting for several hours, and I have on one or two occasions known a patient to feel faint; this too never lasted for more than a few moments, and was never sufficiently severe to prevent my finishing the examination. In some instances an obstruction to the introduction of the instrument is met with low down in the cervical canal. This is not due to any contraction, but to the point of the sound becoming caught in a fold of the mucous membrane, which in this portion of the intra-uterine canal, is not smooth but plaited. Should this occur you must withdraw the point a little, and, altering its direction somewhat, again press it onward. This difficulty is more likely to occur when the os uteri is patulous, and the cervical canal relaxed from the effects of disease, than when it is in a healthy condition; but a little patience and careful manipulation will always overcome these obstructions. I have dwelt at some length on the mode of introducing the sound, because the difficulties of the operation have been much exaggerated, and I am satisfied that these difficulties are mainly due to want of skill on the part of the operator. The method of using the sound which I have described, is that which I always adopt; but there are other modes doubtless equally as good. Thus Dr. Graily Hewitt following the plan recommended by Sir J. Simpson, introduces the index finger of the left hand, guiding the sound along it up to the os uteri. While Dr. West recommends introducing two fingers of that hand for the purpose, the instrument being held in the right hand. But whichever method you adopt, you will speedily with a little practice become adepts, only remember never to use force; better far that you should never introduce the instrument, than you should run the risk of injuring the uterus, and perhaps cause a fatal result, in doing by force what should only be accomplished by tact. But you will frequently meet with cases in which the use of the sound is entirely for-

bidden. Thus, if there be any possibility of pregnancy existing, it would be most improper to introduce it, and you should wait till you are satisfied on this point. In cases of cancer too, and as a rule, during an attack of any form of acute inflammation, your own judgment will warn you against it. But with such exceptions as these I can confidently recommend it to you as a safe and useful instrument—so high is my opinion of the uterine sound, that I make it a rule to introduce it in all doubtful cases unless its use is contraindicated by the possible existence of pregnancy, or by some equally valid cause, and I am satisfied that this will at no distant time be recognised by all well informed obstetric practitioners as the established rule. Now as to the information to be obtained from its use. We learn three things, which it would be impossible to ascertain by any other means. Firstly, we ascertain with positive certainty what the depth of the uterus is. If the sound pass beyond the nodule, at the curve of the instrument, we know that the cavity is unduly elongated, and we can measure accurately the extent to which it is elongated. Secondly, we ascertain the position of the uterus, and determine whether it be in its normal position, or flexed anteriorly or posteriorly. Lastly, we learn whether the entire organ be fixed or moveable—a matter of the greatest moment when we come to decide on the all important question of the nature of some abdominal tumour, the sound, and the sound alone enabling us to decide whether the uterus is engaged in that tumour or not.

But our means of obtaining information are not yet exhausted. Our examination hitherto has been carried on through the vagina. We have ascertained what the condition of the os uteri is. We have measured the depth of the intra-uterine canal with our sound, We are satisfied that the uterus has retained its natural position, or is displaced. But we know nothing of the condition of the external or peritoneal surface of that organ. A fibrous tumour for instance, of any conceivable size may be developed from any portion of the uterine wall, and yet the examination I have hitherto described may fail to detect it. Never omit then in all doubtful cases, to pass the hand over the abdomen, and by the aid of both hands, to satisfy yourself as to the shape and size of the uterus. This method, termed by Dr. Marion Sims the bi-manual method, often affords valuable information. To carry it out pressure is made with the left hand over the pubes, while the index finger of the right is kept in contact with the cervix uteri, the patient lying on her back should be made to expire deeply, and, at this moment the fingers of the left hand should be pressed firmly down into the pelvis, immediately over the pubes while the index finger presses the uterus upward from the vagina. It

will thus, to use Dr. Sims's words, "be easy to measure the size and shape of the body of the womb, for it will be held firmly between the fingers of the two hands, and its outline and irregularities will be ascertained with as much nicety as if it were outside the body." In thin subjects the results here enumerated as attainable can be obtained; but in fat or very muscular women we sometimes fail in our efforts to feel the uterus at all through the abdominal parietes. Still even with these exceptions, the bi-manual method of examination is often of great value.

I have already told you that in order to make an accurate diagnosis it is generally necessary to make a digital examination of the condition of the uterus and vagina, and to use both the speculum and the uterine sound. But in many cases, the two latter modes are not only unnecessary, but positively forbidden. Thus, if on introducing the finger into the vagina you detect cancer of the os uteri, the introduction of the speculum becomes unnecessary, and may be injurious, while the use of the sound is altogether prohibited; or, if on using the speculum we find the os and cervix uteri to be in a state of ulceration, the symptoms the patient is suffering from will probably be accounted for, and the introduction of the sound into the uterine cavity uncalled for, and therefore to be avoided. So your examination in all cases is to be progressive, the finger always being used in the first instance. Any departure from this course, I deprecate strongly.—*Medical Press and Circular, June* 14, 1871, *p.* 501.

76.—ON DILATATION OF THE OS AND CERVIX UTERI.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, Dublin.

Whenever you meet with a case of menorrhagia *in an otherwise unhealthy woman* which a careful vaginal examination proves not to depend on ulceration of the os and cervix uteri, on an extra-uterine polypus, on cancer or such evident cause, to dilate the cervix and os internum with the view of determining what the condition of the interior of the womb may be, I hold to be your manifest duty.

I cannot refrain from quoting the judicious remarks of Dr. Tanner with reference to this subject. He says, speaking of menorrhagia (vol. ii., p. 301),—"When a woman suffers from repeated attacks of uterine hemorrhage which can only be partially or temporarily relieved by rest, nourishing food and proper astringents, we may be sure that there is some organic

disease of the ovaries or uterus; and though the cervix and body feel healthy to the touch, we can be certain that the bleeding is due to some actual disease, that it is not functional." And further on after enumerating what these causes may be he adds, "There is only one plan of treatment which can be adopted with a reasonable hope of success, and that is to dilate the os and cervix thoroughly so as to permit the removal of the source of evil." I fully endorse these observations. There are two methods still practised of accomplishing this object, the one with sponge tents, the other by means of sea tangle. The former can be made of any required size, it is merely necessary to cut a fine clean sponge to pieces conical in shape and of various size and lengths, for you should always be provided with several tents of different sizes before commencing the process of dilatation. You should then wrap each piece as tightly as possible with fine twine commencing at the narrow extremity and winding it on till it reach the thick end. The pieces of sponge should then be immersed in a strong solution of gum arabic, left in it till thoroughly saturated, and then hung up to dry slowly. Before these are used the surface should after the removal of the twine be rubbed smooth. A small-sized tent is to be first inserted, a larger one being introduced on its removal after the lapse of from six to twelve hours. I have entirely given up the use of sponge tents myself: they are troublesome to prepare, give rise to a very fœtid discharge, and are further objectionable because the mucous membrane lining the cervix sinks into the cells of the sponge which is consequently lacerated as the tent is withdrawn, and thereby the risk of inflammation occurring is greatly increased; besides sponge tents from their conical shape necessarily dilate the os externum far beyond what is required before the os internum is opened even to a moderate extent. In fine, sponge tents should never be used if sea tangle can be obtained. Tents made of this substance, technically called *laminaria digitata* have been in use for some years for the purpose of dilating the cervix. The method first adopted was to introduce one which after the lapse of some hours was withdrawn and another of greater calibre introduced in its place, the process being repeated till the os internum was sufficiently dilated. This process was necessarily very tedious and besides objectionable in other points of view. It is now given up and a modification of it introduced by Dr. Kidd of this city adopted in its place. Dr. Kidd's method possesses these three great advantages—that it is comparatively rapid, that it is cleanly, and lastly and most important of all that it dilates the canal equally throughout its whole length, except in some cases of rigidity of the os internum to which I shall allude presently.

Having decided to dilate the cervix the first step is to expose the os uteri by means of the duck-bill speculum, next to seize the anterior lip with a small hook and with it to draw down and steady the uterus. You should previously measure the depth of the uterus and have ready several pieces of sea tangle bougies, each piece being at least the length of the uterine cavity. These you now proceed to introduce, the main difficulty is nearly always with the first, and this difficulty is greatly increased if the uterus be retroflected. The short lengths not being so easily manipulated as longer ones I sometimes when difficulty occurs take an entire bougie and pass it through the os internum as I would the sound. I then slip pieces of the proper length in beside it, for when we have inserted one piece it straightens the uterus and serves as a guide to the others. When several pieces have been introduced you can withdraw the long one, or if before passing it you nick it round at a point corresponding with the length of the other pieces you may be able to break it there, and so avoid the trouble of having to substitute another length in its place. The number of pieces you should insert varies in each case. If the patient have never been pregnant and the cervix rigid you will not be able to get in more than three or four, but if she have borne children or if the cervix be relaxed you may succeed in introducing double that number, or even more without difficulty.

If a small number only have been introduced, it is better to withdraw them after the lapse of nine or ten hours and introduce a larger number; but if seven or eight pieces have been inserted they may be left for twenty-four hours before any further steps be taken. The sea tangle rapidly absorbs moisture from the vagina and uterus, and swells, and by swelling forcibly dilates the cervix. This of course causes pain which however is seldom very severe and generally passes off after a few hours. If it continue I usually direct a morphia suppository to be introduced into the rectum, or 20 grains of the hydrate of chloral to be administered at bed-time. Dr. Graily Hewitt who still advocates the use of the sponge tents in preference to the sea tangle, states, as an objection to the latter that it is liable to slip out. This certainly is true if you use the short tents which are sold in boxes, but if you use pieces of the bougie of the length already specified and take care that they pass up to the fundus, there is very little chance of them being expelled; on the contrary I have on two or three occasions experienced some difficulty in removing them. This has been the case when the os internum was so rigid, that it prevented the sea tangle expanding as freely at that point as it did in the cavity of the uterus and in the cervical canal; and the pieces of tangle being thus constricted in the middle, it

was necessary to press the index finger of the left hand firmly against the lip of the os uteri, while with a pair of long forceps, held in the right hand, one piece is seized and slowly extracted. These are the cases in which, as just mentioned, the whole extent of the canal is not equally dilated, and then fresh pieces of the tangle must be introduced, and time given to allow of them to expand, before proceeding to explore the interior of the uterus. You will, however, from time to time meet with cases in which, although the sea tangle has expanded to its fullest extent, still, from the size of the tumour, or some other cause, the os internum is not as large as you would desire. Under such circumstances, I usually complete the process by the introduction of one of Dr. Barnes's dilators. These are India-rubber bags, of a somewhat hour-glass, or rather fiddle shape. They are made of three different sizes. One end terminates in a long slender tube, the extremity of which is furnished with a stop-cock. The dilator is introduced in a flaccid state into the uterus on the point of a staff or sound, the terminal bulging part being carried through the os internum, and air or water being then gradually forced into the dilator through the long tube just alluded to, it is left for an hour or two, and by that time has generally distended the canal to a considerable extent. The peculiar shape of the dilator prevents it, when once it has been distended, from slipping out of the uterus. Dr. Barnes originally introduced these bags into practice for the purpose of dilating the os uteri in cases in which it was desirable to induce premature labour, a purpose which they often serve admirably, but their use is now further extended, and we use them occasionally for the purpose of completing the dilatation of the cervix in the unimpregnated uterus.

You have had frequent opportunities of seeing the process I have described carried out—and must have noticed the entire absence of unpleasant symptoms, after a proceeding so apparently severe as the forcible dilatation of the cervix uteri—I have, therefore, no hesitation in recommending you to adopt this course in your future practice, as being one which you have seen productive of such good results in this hospital.

I have now explained to you the mode by which dilatation of the cervix is to be accomplished. It remains for me to direct your attention to the way in which you are to proceed, when having withdrawn your sea tangle or sponge tents, you desire to clear up the doubt which exists, and satisfy yourself as to the cause of the menorrhagia. Attempts have been made to examine the interior of the uterus by means of the endoscope; but as yet with negative results, the hemorrhage which in these cases is always present to at least some extent, and the

mucous discharge which is continually being poured out, prevent anything being distinctly seen. We are therefore obliged to rely on the sense of touch alone, and must therefore pass the index finger fairly through the os internum till the tip reaches the very fundus. To accomplish this by no means easy matter, it is necessary, in the first instance, to draw down and fix the womb, this you effect by seizing the anterior lip of the os uteri with a vulsellum, which you intrust to an assistant to hold, while the fundus should be at the same time pressed down by your left hand, or, better still, by another assistant, the finger well oiled is now introduced slowly through the os internum, and swept round the entire cavity of the uterus. You will thus detect the existence of a polypus, or a tumour, no matter how small, should either be present, while the educated finger will recognise the rough uneven feel which the mucous membrane, if in an unhealthy granular condition, conveys to the touch.—*Medical Press and Circular, July* 26, 1871, *p*. 72.

77.—ON MECHANICAL DYSMENORRHŒA.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery King and Queen's College of Physicians, Dublin.

[There are three varieties of mechanical dysmenorrhœa. Firstly, that in which the cervical canal is so flexed as to obstruct the escape of the menstrual discharge; secondly, that in which inflammation or congestion of the lining membrane exists to such an extent as to cause temporary closure of the canal, or of the os internum; and thirdly, that in which there is some congenital narrowing of the os internum or cervical canal.]

Painful or difficult menstruation is frequently met with in women in whom the uterus is flexed; but though flexions of the uterus may and certainly do sometimes interfere with the exit of the menstrual flow, they certainly seldom do so unless the flexion be complicated by the existence of chronic inflammation, or the presence of a fibroid. In such case we should certainly endeavour to relieve the flexion, and see if by replacing the fundus in its normal position, and supporting it there by a pessary, we can relieve the patient, before having recourse to surgical means, which are less suitable in this than in any of the other forms of mechanical dysmenorrhœa. I have already so fully explained my views as to the chief cause of the dysmenorrhœa in cases of inflammatory swelling of the lining membrane of the uterus, that I have but to repeat that though not in my opinion of very frequent occurrence, cases are met with in which the os internum or some portion of the cervical

canal becomes so narrowed in consequence of the tumefaction of the parts as to present a mechanical impediment to the discharge of the menses; in such cases I have no hesitation in having recourse to surgical treatment with the view of procuring relief; indeed it is obvious that an operation which divides the cervix so freely as does that introduced by Sir James Simpson must be calculated to give permanent relief to the congested organ. I only say again that the operation should not be had recourse to till other means have failed, including the dilatation of the cervix by means of sea tangle tents. I may here take the opportunity of saying once for all that I unhesitatingly condemn the use of any of the metal instruments which have been suggested for the purpose of dilating the cervix; their use is attended with danger, as they act too rapidly and sometimes rupture the uterine fibres, several cases of severe inflammation and even of death are on record as following their use, while the sea tangle is perfectly harmless. A contracted os, looking almost like a pin hole and leading up to a narrow cervix uteri, is not unfrequently seen; this condition is almost invariably associated with sterility, and very often with dysmenorrhœa also. You saw last week a very good example of this in the case of the young woman who sought relief for the latter affection. Menstruation is with her both painful and scanty, the os uteri so small as hardly to admit the point of a probe; there is no doubt the cervical canal is in this case unduly contracted. I think such cases as hers are fair subjects for operation, for no other treatment will be productive of permanent benefit, but beware of holding out hopes to your patient that, by submitting to the operation she will gain more than relief from the suffering caused by the dysmenorrhœa, the operation has been proposed and performed for the relief of sterility. As far as my experience goes it has resulted in failure. In other words it is in my opinion a legitimate proceeding, if performed with the view of curing cases of dysmenorrhœa, in which other treatment has failed, or is inapplicable, but that it is not warranted in cases of sterility, because the narrow os and contracted cervical canal are not the cause of the sterility, but merely an index of some congenital condition or defect in the uterus itself which hinders conception; what that defective condition may be we do not at present know.

But the patient I have first alluded to is averse to undergoing any operation, and I have therefore introduced a slender and short stemmed galvanic pessary. She has worn it for a fortnight and now tells you that she feels easier. You saw that I had some difficulty in introducing it mainly because the uterus is slightly ante-flected. I had accordingly to expose the os with

the duck-bill speculum, then to seize and draw down the cervix with a fine hook, and while the womb was thus fixed to slip in the stem of the pessary. You must always adopt this method when difficulty occurs in the introduction of these instruments. I have known much good to result in such cases as the foregoing from this simple treatment, it is at least worth trying before advising that an operation should be performed. .

Now with respect to the operation itself, we are indebted for its introduction to Sir J. Simpson, who for a time practised it very extensively, though I believe that before his death his views on this point were considerably modified, and that he did not perform it nearly so frequently as he had done at an earlier period of his career. His method of performing the operation was by passing an instrument termed a bistourie cachée through the canal of the cervix and within the os internum. It contained but one blade, which, when the instrument had penetrated to the requisite depth was made to protrude, the extent of the protrusion being regulated by a screw; the incision commenced at the os internum, and as the instrument was withdrawn it incised gradually and more deeply the substance of the cervix, till it divided the vaginal portion quite through; the instrument had then to be turned, reintroduced, and the other side divided in like manner; this re-introduction is very objectionable, and consequently various knives have been invented with the view of obviating it. Those proposed by Dr. Savage and Dr. Greenhalgh are both good instruments. I give the preference however to the former. It is furnished with two blades, the cutting edge of each being directed outwards, but as the back of each blade when the instrument is closed projects beyond the cutting edge of its fellow which it thus overlaps, its introduction into the cervix can be safely effected, but it is generally necessary to dilate the cervical canal before this step can be effected. This indeed is generally necessary no matter what instrument is used, two pieces of sea-tangle will however open the canal sufficiently for the purpose; you should then, having exposed the os by means of the duck-bill speculum, proceed to introduce the knife slowly, taking care that it does not pass unnecessarily far into the uterus, the blades should then be expanded slowly and only to a very limited extent at first, for if this precaution be neglected you will divide the os internum too deeply, a proceeding which may cause alarming hemorrhage, and is nearly certain to be followed subsequently by such excessive eversion of the lips of the womb as to leave the neck patulous and gaping to an extensive degree; this condition exists in a patient at present under my care who was operated on more than ten years ago. When the blades are clear of the os internum they are to be expanded more widely, but I think it

safer not to divide the vaginal portion of the cervix with them, but to complete this part of the operation subsequently by means of a pair of curved scissors. Dr. Marion Sims varies the operation by dividing first one and then the other side of the vaginal portion of cervix with a pair of bent narrow-bladed scissors, he then passes a narrow-bladed knife through the os internum and cuts from within outwards. The operation of dividing the cervix uteri is not devoid of danger; it has, though not in my practice, been followed by fatal results. I have known very alarming hemorrhage to occur, both at the time and also some hours subsequently. You should therefore be always prepared for this contingency, and be provided with a solution of the perchloride of iron in glycerine; with this you should invariably brush over the divided surface, and should hemorrhage occur, insert a pledget of cotton saturated with it into the cervix, and then plug the vagina. The operation itself seldom causes pain, and if the woman be healthy the chance of inflammation following is not great; still extreme care should be taken to guard against such occurring, and she should be kept in bed for several days. There is often a great tendency in the incisions to unite. To prevent this Dr. Coglan has suggested the insertion of a thin roll of lead; this answers the purpose very well. It is sometimes necessary to introduce and leave in the cervix an expanding spring tent, but not unfrequently these precautions may be dispensed with. Dr. Graily Hewitt recommends with the view of preventing contraction, and at the same time of keeping the canal straight, that the patient wear for some time subsequently an ebony stem pessary, a proceeding which in many cases would doubtless be useful.—*Medical Press and Circular, July* 5, 1871, *p.* 3.

78.—OBSERVATIONS ON THE TREATMENT OF SOME FORMS OF MENORRHAGIA.

By Dr. LOMBE ATTHILL, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, and Obstetric Physician to the Adelaide Hospital, Dublin.

[The subject of the etiology and treatment of the various cases of menorrhagia is a most extensive one. In the present paper Dr. Atthill confines himself to the consideration of the treatment most suitable to cases of monorrhagia when occurring in connexion with, or dependent on, subinvolution of the uterus, on granular ulceration of the cervix, or on an unhealthy condition of the mucous membrane lining the body of the uterus.]

Defective involution of the uterus after labour or abortion

occupies a prominent place among the causes giving rise to excessive menstruation. That this should be the case is but natural; for not only is there, when subinvolution exists, an undue amount of blood present in the organ, but also the relaxed condition of the uterine tissue favours its exudation, and, therefore, when the periodic determination of blood to the uterus takes place, as it occurs at each menstrual period, the moderate flow which should relieve that congestion becomes a profuse discharge, and often an exhausting drain. But the mischief resulting from subinvolution does not end here, for that abnormal state of the uterus predisposes to that unhealthy condition known as "granular ulceration" of the os and cervix uteri: a condition in which the mucous membrane of the canal of the cervix is hypertrophied, becomes exceedingly vascular, and is often everted to a considerable extent: a condition which increases the previously existing tendency to hemorrhage. Thus, in not a few cases, do we find the two causes present in the same patient. The following case affords a well-marked instance of this.

Mrs. F., aged 48, the mother of twelve children, presented herself among the extern patients of the Adelaide Hospital nearly a year ago. She stated that ever since the date of her last confinement, five years since, menstruation had gradually become more profuse, the flow continuing for a longer time than usual, the intervals between the periods being correspondingly shortened. During the intervals she suffered much from profuse leucorrhœa, and, as a result of this nearly-incessant drain, became greatly debilitated. On examining the uterus, the sound passed to the depth of three inches and a half. The os uteri was patulous; there was extensive ulceration of the os uteri; and, on separating the lips, the mucous membrane lining the cervical canal was seen to be thickened and highly vascular. This was a case requiring energetic treatment, which I did not dare to attempt so long as she continued to follow her ordinary occupations; and, for a time, she refused to come into hospital. But, finally, her condition becoming much worse, she consented to do so. My first step after her admission was to introduce several lengths of sea-tangle bougies through the os internum, with the view of being able to explore the cavity of the uterus, and to make the necessary applications to the entire extent of the cervix uteri. This proceeding having enabled me to decide that there was not any polypus or fibrous tumour in the uterus, I cauterised the entire of the inner surface of the uterus freely with the strong nitric acid; this did not cause any pain. She was, however, as a precaution, kept in bed for three or four days. On examining her after the lapse of a week, the condition of the ulceration

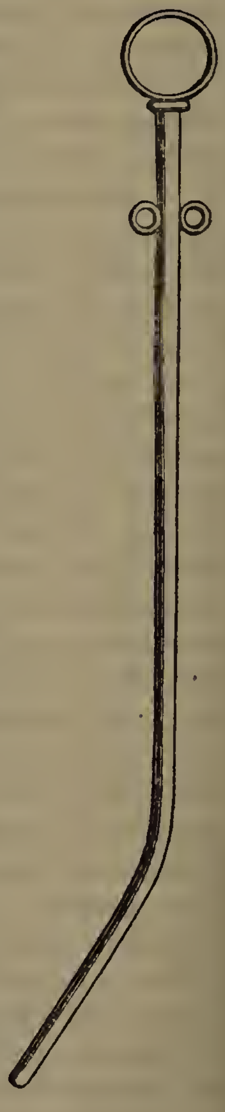
which existed round the lips of the os was found to have improved considerably, and she was discharged in a short time perfectly cured.

In the foregoing case, subinvolution was manifestly the primary cause of the menorrhagia, the ulceration being altogether secondary. But, in many cases, subinvolution exists alone, or on the other hand, ulceration may exist alone, either condition being fully sufficient to give origin to severe menorrhagia. As an instance of the former, the following serves as an example. F. L., aged 24, married about a year, was a delicate young woman of lymphatic temperament. Menstruation had always been profuse, especially if she took walking exercise, or exerted herself during the flow. She became pregnant after the occurrence of the second menstrual period following her marriage, but, having imprudently taken a long and fatiguing walk, she aborted at about the eighth week; the two subsequent menstrual periods were so profuse as to reduce her to a state of extreme debility. Ergot, gallic acid, &c., failed to do good. On examining her after the termination of these periods, the uterus proved to be considerably elongated, the sound passing to the depth of three inches and a half; there did not exist any ulceration. The history of the case being altogether against the supposition of the existence of polypus, I came to the conclusion that the menorrhagia depended on subinvolution; in fact, that the uterus had never regained its normal size and tone since the miscarriage, which had occurred two months previously. I therefore decided on carrying out a plan of treatment, the value of which I have repeatedly tested: I mean the introduction up to the fundus of the uterus of ten grains of the solid nitrate of silver, and leaving it to dissolve there. This I accordingly did. The application produced considerable pain, which lasted five or six hours, but no further unpleasant results followed. I confined this patient to bed for several days, but then allowed her to go about. Menstruation appeared at the regular time, and was moderate in quantity; and she became pregnant immediately afterwards.

I wish to call attention especially to this case: first, as illustrating the occurrence of subinvolution as a result of abortion—a fact which is overlooked by many; next, as showing the dangerous menorrhagia that may depend on this condition of the uterus; and, thirdly, as proving the excellent results which follow the treatment adopted. Ergot, gallic acid, and, indeed, all other medicines will frequently fail to check menorrhagia depending on subinvolution; and we must have recourse to treatment directed to the uterus itself; we must stimulate the organ to set up that healthy action by which it regains its normal size after pregnancy has terminated—a pro-

cess to which Sir J. Simpson has applied the term "involution." With this view, I unhesitatingly advocate the adoption of the treatment practised in the preceding case. I know no other so efficacious.

The mode of carrying it out is simple. The instrument known as Sir Jas. Simpson's "*uterine porte caustique*" is introduced into the uterus just as an ordinary uterine sound. This little instrument consists of a hollow silver tube, in size and shape closely resembling a sound; it contains a flexible stilette, which it fits accurately. As soon as its point is found to have reached the fundus of the uterus, the stilette is withdrawn, and through the instrument is pushed up, by means of the stilette, a piece of solid nitrate of silver reduced to the requisite size and weight, till it is fairly lodged in the cavity of the uterus. In doing this, there is but one caution requisite to be attended to; namely, that as soon as the piece of nitrate of silver has reached the extremity of the *porte caustique*, and before it is finally pushed out of the instrument—a point of which we can always be certain by observing how much of the stilette remains still unintroduced—the instrument should be withdrawn to the extent of about half an inch; for if this precaution be not observed, it is possible that the nitrate of silver might be forced into the substance of the uterine wall, instead of being left free in its cavity, an accident which, though possible, is very unlikely to occur. I have dwelt at some length on this plan of treatment, because I am satisfied that its value is far from being appreciated. It is looked upon by many practitioners as heroic and dangerous. I believe, and I have practised it for several years, that it is simple and safe. I do not say that it is always sufficient, and that a cure must always result; but in my hands it has been productive of marked success, and in no single instance have I known of its producing serious symptoms. The application of solid nitrate of silver to the interior of the uterus is by no means a novel practice. Dr. Evory Kennedy used to introduce it into the cavity of the body of the womb by means of an ordinary female catheter several years ago, but he did not leave it there to dissolve. To Sir J. Simpson we are indebted for the further development of the practice, and



for the invention of the *porte caustique.* Dr. Kidd, I think, was the first of the Irish obstetricians who adopted this practice.

Menorrhagia resulting from ulceration of the os and cervix uteri is also of frequent occurrence. Mere abrasion of the lips of the os uteri is not sufficient to produce menorrhagia; but that unhealthy, spongy condition of the os and cervix, in which the mucous membrane lining its canal becoming hypertrophied and thickened, and bleeds on the slightest touch, the os being patulous and the lips everted, is quite capable of originating severe menorrhagia.

Mrs. B., a young married woman, aged 24, who had never been pregnant, stated that she had become greatly debilitated by the excessive loss which occurred at each menstrual period. Ergot and astringents were exhibited by the mouth, and astringent lotions injected into the vagina, without producing the least effect. The use of the speculum proved the existence of extensive granular ulceration of the os and cervix uteri. Now, in severe cases, such as the one I am referring to, the unhealthy condition of the mucous membrane extends at least as high as the os internum, and we will fail to effect a cure unless our treatment reach every portion of the diseased tissue; therefore, with the view of permitting the necessary applications to be made to the whole extent of the cervical canal, I commenced my treatment by introducing two tents of compressed sea-tangle; two pieces being sufficient for the object I had in view, which was not to open the uterus to such an extent as to enable me to examine its cavity, but only to permit me to treat the entire of the cervical canal. I left these pieces *in situ* for twenty-four hours, and on withdrawing them after the lapse of that time, cauterised freely the whole diseased surface with fuming nitric acid. This did not cause any pain. On examining the os uteri a few days subsequently, I found it in a much healthier state. *The menorrhagia was entirely checked, and never returned;* and, although a considerable time elapsed before the uterus regained a healthy state, still the progress of the case was rapid and the cure perfect; the only treatment subsequently necessary being the occasional application of a twenty grain solution of nitrate of silver to the os uteri, and, at a later period, of a small blister to the sacrum; finally, not the slightest trace of the ulceration remained, and menstruation became in all respects normal.

The foregoing case illustrates perfectly the mode of treatment which I, as a rule, adopt in cases of granular ulceration of the os and cervix uteri.—*British Medical Journal, June* 24, 1871, *p.* 666.

79.—ON THE VARIOUS METHODS OF TREATING THE PEDICLE IN OVARIOTOMY.

By Dr. D. Lloyd Roberts, Vice-President of the Obstetrical Society of London, and Physician to St. Mary's Hospital, Manchester.

[It is now universally conceded that the first *bonâ fide* operation for the removal of an ovarian tumour was performed by Dr. Ephraim MacDowell, of Danville, Kentucky, U.S., in December, 1809. Mr. Duffin, of London, was the first to recommend and practice the plan of bringing the pedicle through the wound and fixing it with sutures to the abdominal parietes. Mr. Duffin's first case occurred in 1850.]

Mr. Erichsen introduced an important improvement into our practice in 1853. In a case operated on by him, an account of which is given in the *Lancet* (1853, vol. ii, p. 611), the pedicle was tied in two portions, and the abdominal incision brought together by means of sutures closely applied, with two harelip pins at the lowest part of the incision, the ligatures of the pedicle being twisted round the harelip pins in such a manner that the pedicle was secured outside the abdomen between the lowest pin and the inferior angle of the wound. In 1857 Dr. W. L. Atlee, of Philadelphia (*Gardner's Scanzoni*, p. 255), first employed the écraseur in the division of the pedicle, and applied the perchloride and persulphate of iron to all bleeding vessels, thus doing away with the necessity for the use of ligatures. The next and perhaps the greatest advance in the treatment of the pedicle we owe to the ingenuity of Mr. Jonathan Hutchinson, who, anxious to avoid the use of ligatures and the risks thereby induced, devised the *clamp*, an instrument for the permanent compression of the pedicle until detached by a process of sloughing. The first clamp used by Mr. Hutchinson was a pair of carpenter's callipers; this he used in two cases, both of which were successful. He was soon after followed by Mr. Spencer Wells, who eagerly adopted the new method, and in whose hands it has led to such brilliant results.

The introduction of the clamp dates from 1858, since which there have been numerous and varied modifications of the original form: to a few of these we must now refer. Shortly after using the callipers Mr. Hutchinson devised his clamp. About the same time Mr. Spencer Wells devised his improved clamp, which is composed of two parallel blades provided with two screws for fixing, and which is figured in his paper entitled Cases of Ovariotomy, (Dublin, 1859, p. 36); this he continued to use until 1869, when he devised his circular clamp, which is figured in the *Medical Times and Gazette* (Oct. 30, 1869, vol. ii, p. 530), as made by Mayer and Meltzer. The chief objection to

this clamp is that it is liable to cut; I found this in one of my own cases. There is nothing to prevent its edges being rounded off, however. He has since discarded the circular clamp, and now uses a modification of the original calliper type, with the upper and lower surfaces of its blades bevelled and rounded off, which is made by Krohne and Sesemann. This is an excellent clamp, and possesses many advantages over his former one. It lies more comfortably on the abdomen, and takes up less space, from its being curved with the concavity upwards; the pedicle is thus more easily dressed and the discharges are prevented from accumulating round it.

[The next method introduced in the treatment of the pedicle was that of Mr Baker Brown, which consists in a combination of crushing and cauterization.]

Sir James Simpson proposed acupressure for securing the ovarian pedicle in January, 1860 (*Edin. Med. Journ.*, vol. v., p. 649). He has described his method of procedure in the *Medical Times and Gazette*, 1860, vol. i., p. 285, and also in his work on *Acupressure* (Edinburgh, 1864), in which he reports a case illustrating its successful application (p. 439). A modification of this method was proposed by Mr John Dix in 1864 (*Edin. Med. Journ.*, vol. x., p. 216), who thought that the disadvantages inseparable from the use of the needle as a compressing agent might be obviated by the substitution of a fine wire of silver or iron for the needle. In 1866 Dr. J. F. Miner (*Buffalo Med. Surg. Journ.*, vol. v., p. 432) suggested the plan of applying "a ligature to the pedicle as a whole, carrying its extremities through the vaginal septum, and in this way secure a drain for any purulent infection that might take place;" this he carried out in September, 1866, but, unfortunately, the case terminated fatally.

Dr. Graily Hewitt (*Brit. Med. Journ.*, 1870, vol. ii., p. 458) has contributed another to our stock of mechanical contrivances for treating the pedicle. His instrument [see woodcut in *Retrospect*, vol. lxiii., p. 292] consists of "a framework of steel, shaped something like a shoe-buckle, measuring $2\frac{1}{2}$ by $1\frac{3}{4}$ inches . . . . and is provided with studs or buttons eight in number, three on two sides and one on each of the other two sides." It is used thus: the pedicle is perforated by a needle, bearing a double ligature of strong thread or whipcord in two or three places, according to the width of the pedicle; it is then tied in segments, the opposite ends being secured one by one to the framework which is now made to surround the pedicle, the cut edge of which is freely open to inspection and treatment.

The most recent and, apparently, a most effectual method of *dividing* the pedicle is that introduced to the profession by Dr,

G. H. B. Macleod, of Glasgow (*Lancet*, 1871, vol. i., p. 108). It may be viewed as a combined process of crushing, compression, and twisting. The pedicle is seized with a sort of clamp forceps, with narrow, small, and female blades, which are thus capable of maintaining a very firm hold, their apposition being kept up by means of a screw. After cutting away the cyst at the extreme distal end of the pedicle, the stump is seized with another pair of stirrup-shaped fenestrated forceps, also provided with quadrant and screw; this latter instrument being slowly turned, while the first-named forceps is maintained at rest, the stump of the pedicle is by this means twisted off close to the portion held by the first forceps. Mr. Hilliard has also made for Dr. Macleod an ingenious clamp of the calliper type.

Dr. Washington L. Atlee, of Philadelphia, has provided us with the most recent clamp, for which he claims the following advantages:—It is much smaller, lighter, and stronger than the older forms of clamp, and by its use the pedicle may be compressed in the smallest possible space, and what is of permanent importance in the *linear* direction of the wound, the direction most favourable for the approximation of its edges. I am indebted to Messrs. Krohne and Sesemann for an inspection of this clamp, and can testify to its great lightness, combined with elegance and strength.

The great objects in the treatment of the ovarian peduncle are:—1, to prevent hemorrhage; 2, to avoid peritonitis; 3, to avoid the risk of purulent infection (septicæmia, pyæmia) from the absorption of dead or decomposed animal tissues; 4, to avoid undue dragging on the uterus and its appendages; and 5, generally to avoid everything likely to interfere with the healing of the abdominal wound or to retard the recovery of the patient. All the methods devised for the attainment of these objects may be ranged under three heads, according as the stump of the pedicle is left within the cavity of the abdomen, or is partly within and partly without ("pocketed"), or is retained outside the abdomen; they may be termed the intra-abdominal, intermediate, and extra-abdominal methods.

The various methods of securing the pedicle so that it may be returned into the abdominal cavity comprise the use of the ligature, compression, torsion, cauterization, or crushing of the vessels, or a combination of two or more of these means, together with "capping" and "enucleation."

The ligature has consisted at various times of leather (N. Smith), hemp, silk, silver or iron wire, or catgut. It may be applied to the pedicle *en masse*, or one or more double ligatures may be employed, or each vessel may be ligatured separately. The ends of the ligature or ligatures may be cut short, or they may be brought out by the abdominal wound, by the fovea

inguinalis interna, or by the vaginal septum. The practice of cutting short the ends of the ligature was rescued from disuetude by the energy of Dr. Tyler Smith, who in the year 1861 gave fresh impetus to this method. Certainly this practice is a most attractive one, as if the ligature becomes sacculated or remains innocuous, the case progresses rapidly to a favourable termination. For apart from the immediate shock of the operation and the risk of peritonitis, the greatest anxiety in these cases is felt lest there should be excessive suppuration caused by separation of the pedicle, strangulated vessels, and adhesions; this being got rid of, the case becomes one of a simple incised wound in the abdominal wall, which rapidly heals. I recently (June 28th, 1871) removed an ovarian cyst and returned the pedicle with the ligature cut close; the wound in the abdomen healed by first intention, and the patient had not a single unfavorable symptom; she might be considered quite well by the tenth day after the operation. In one case which terminated favorably, Mr. Bryant, a very successful ovariotomist, reports (*Guy's Hosp. Reports*, 3rd series, vol. xiv, p. 228) that the ligatures which secured the peduncle came away through an artificial anus at the lower part of the abdominal wound twenty-five days after the operation; he also mentions two other cases which terminated favorably, the silk ligatures by which omental adhesions had been secured having been discharged externally through the wound. On the other hand, he mentions a fatal case in which "some half-dozen ligatures had to be employed, and were left in," and on post-mortem examination the ligatures were found in the abdominal cavity "resting in their own depôts of pus, having been thrown off from their attachments, and acting as foreign bodies." Notwithstanding these occasionally unfavorable cases, the practice is generally sound and productive of most favorable results. I am informed by Dr. Braxton Hicks that "the preference here (Guy's) is for tying and dropping in the pedicle."

On the other hand, when this method is employed, we know that adhesion takes place between the pedicle and the neighbouring gut, and in some cases circumscribed collections of pus have formed, until at length it finds an outlet either through the abdominal wall, the vagina, or the intestine.

Where the ends of the ligatures are brought out by the abdominal wound, the chief objections are that there is much greater probability of the strangulated portion of the pedicle becoming gangrenous, and of putrid infection as a consequence—there is also greater risk of peritonitis. Another objection is the length of time required for the separation of the ligature—I have known it to be thirty days. This retards convalescence, confines the patient to bed, and is a source of anxious care to

the surgeon; moreover, it weakens that portion of the abdominal wall through which the ligatures pass. Another great objection to its employment is the tendency to the formation of fæcal fistulæ; "a sort of canal or sinus is formed by the adhesion together of folds of omentum or coils of intestine, in such a manner as to enclose the ligature and shut it off from the general peritoneal cavity" (Spencer Wells, in *Glasgow Med. Journ.*, Feb., 1868). This occurred in the case of a lady upon whom I operated in June, 1866. On the fifteenth day a portion of intestine opposite the lower part of the wound gave way, and fæcal matter passed continuously through the wound for some weeks. The perforated intestine, however, eventually healed, and a good recovery took place.

[*Compression* may be applied to the pedicle by means of acupressure (Simpson), the wire compress (Dix), the serre-nœud (Kœberle), and the coil-clamp (Aveling).]

Torsion may be applied to the vessels of the pedicle after division (Beebe). This is a very excellent plan, and has been very successful in the hands of its introducer. It is well worthy of a farther trial.

With reference to the method proposed by Dr. Macleod, if further experience should confirm the report given by him of the remarkable immunity from hemorrhage, and from constitutional disturbance of the patient whose successful case he quotes, it will speedily take a foremost place in our treatment of the ovarian peduncle. I shall certainly make a trial of his method in the first suitable case that presents itself to me. [For woodcut and description of Dr. Macleod's instruments see *Retrospect*, vol. lxiii., p. 296.] I cannot help thinking that it would be an improvement if his clamp forceps were made with parallel blades, as by this means the pedicle would be compressed on all its surface equally; as it is, the clamp forceps acting like a pair or scissors, compresses the pedicle unequally. The chief objections to the use of the *écraseur* are: the time required to separate the tumour, that it frequently fails to arrest the hemorrhage, and that it gives rise to as much suppuration as any of the other methods. Dr. Atlee, its introducer, has now discarded it.

I take it as a fundamental principle that no one method of securing the ovarian pedicle is of universal application; each case must be treated according to circumstances, and the multiplicity of methods furnishes us with ample choice. If I were asked how I should treat the pedicle, I should answer somewhat after the following fashion:—If the pedicle were long, narrow, and could be easily brought outside without traction upon the uterus or making any undue pressure upon the wound at the lower part of the belly, I should most certainly use the

clamp; if, on the contrary, the pedicle were short, and I feared dragging or too much displacement of the uterus, and if it were not too voluminous, then I should transfix it with a firm ligature crossed so that it could be tied on one side only, cut the ligature close, and drop the stump into the pelvis. In cases where the pedicle could neither be safely brought out with the clamp nor ligatured for fear of vessels retracting or tissues shrivelling up; or where the pedicle was very short, vascular, and fleshy, and I feared the ligature might cut it; or where it was very close to the uterus, I should secure it with a clamp (moderately crushing it), divide it, secure each vessel with a ligature cut short, and apply the actual cautery to the rest of the stump, waiting a few minutes to see if any oozing of blood took place before dropping it into the pelvis.

If the pedicle were very voluminous, the cyst having a very broad attachment to the uterus, so that I could not use the clamp to hold and crush it whilst I applied the cautery, I should divide it with a pair of blunt scissors slowly, taking up each vessel as I went on, and apply the cautery to the remaining portion of the stump.—*Manchester Medical and Surgical Reports, Oct.* 1871, *p.* 94.

80.—ON THE USES AND APPLICATION OF HODGE'S LEVER-PESSARY.

By Dr. Robert Barnes, Obstetric Physician to St. Thomas's Hospital; Examiner in Midwifery and the Diseases of Women to the University of London, the Royal College of Physicians, and the Royal College of Surgeons, &c.

[Hardly anything in uterine affections is less understood than the value of this instrument in cases of retroflexion and retroversion of the womb, and we are consequently glad of the opportunity, afforded by Dr. Barnes's clinical lecture on the subject, to bring it before our readers.]

The morbid condition which the lever-pessary is especially adapted to relieve, is retroversion and retroflexion of the uterus. The more we reflect upon the great frequency of this condition, the distress it entails, and the general efficacy of the instrument in curing it, the better shall we estimate the debt of gratitude under which our distinguished American brother has placed us by putting this admirable contrivance in our hands.

It is right to premise that Professor Hodge extends the application of his lever-pessary to many other morbid conditions besides retroflexions of the uterus. I would not so much as insinuate that the Professor has ever so little given the reins to his hobby as to exaggerate the uses of his invention. I know

from experience that it is often of great service in chronic metritis, especially in inflammation with engorgement and hypertrophy of the cervix, in vaginitis, and several other diseases. By counteracting prolapsus, by securing comparative "rest" of the organ, by maintaining it at its proper elevation, it tends remarkably to counteract congestion, to restore freedom of local circulation, and thus to promote the cure. But the special subject of the present lecture is retroflexion of the uterus. I will not now discuss the causes of this displacement; they form the subject of a controversy which will probably be carried on for some time to come. One camp contends that it is a primary condition entailing congestion, inflammation, and other evils; another camp contends that it is secondary, and produced by the congestion or inflammation, which is often found associated with it. The rational course is to look at both sides of the shield. I have no doubt that retroflexion follows labour. The uterus remains bulky from imperfect involution, and the heavy fundus is driven back by the superincumbent pressure of the intestines; it may be the consequence of adhesions, the reliquiæ of peritonitis. The body of the uterus may in like manner be bent backwards by a tumour in its walls, by its bulk being increased by hyperæmia or hypertrophy, or by other morbid conditions. But close observation has convinced me that primary retroflexion is far more frequent. I meet with it frequently in women who have never had children or sexual relations. In fact, it is a frequent cause of sterility. In most, if not in all, of these cases I believe that the retroflexion is congenital. Until the advent of puberty, little or no inconvenience is felt; there is nothing to draw attention to the existence of the displacement. But when menstruation sets in, when the uterus under the ovarian stimulus grows and undergoes periodical engorgement, then trouble begins. The flexion at the neck aggravates beyond the physiological degree the menstrual hyperæmia of the body; the excretion of the menstrual discharge is impeded; a severe form of dysmenorrhœa becomes established; the bladder may become irritable; the enlarged body of the uterus projects into the rectum in the hollow of the sacrum, obstructing its canal like a ball-valve. Hence constipation, and gradual retrograde or ascending difficulty invades the whole alimentary canal; a degree of paralysis of the intestines ensues; coprostasis leads to decomposition of fæces in the intestine, to flatulence; then other dyspeptic evils follow: loss of appetite, attended, perhaps, by vomiting under the pain of dysmenorrhœa, blood-degradation, general impairment of nutrition. These surely entail disorder of the nervous system. Henceforth the patient's life is one of constant, or, at best, of intermittent suffering, under which she may break down

altogether. You may exhaust the Pharmacopœia in treating the stomach, the liver, the brain, or the spinal cord; but so long as you leave the cause untouched, the patient will continue to suffer.

How are you to detect this cause? Let us first enquire what are the symptoms which indicate uterine or ovarian disorder? what are the symptoms which justify physical exploration of the pelvis? If you see dysmenorrhœa to a severe degree, attended or not by menorrhagia, followed by leucorrhœa; if you learn that the sexual function is endured with difficulty; if you see the patient's general health becoming impaired under the repeated suffering; and if you learn that ordinary general and local treatment, fairly tried, have failed to bring relief—I think myself, and I know that most patients will be of the same opinion, that you ought to ascertain the condition of the organs whose functions are performed with so much distress. The time has come to give up the vain chase of the disease through its consequences, and to apply our attention directly to the organs which are obviously at fault. I admit that general means, medicinal and hygienic, should have a fair trial. Not seldom dysmenorrhœa is relieved by time and mitigated by medicines; but, on the other hand, more often still, dysmenorrhœa and its consequent ills are intensified by repetition, until the local condition and the remote disorders, working in a vicious circle of action and reaction, become so firmly established as to imperil the success of any treatment. We assume, then, that the case calls for full clinical investigation. The finger and the uterine sound are the instruments of diagnosis. Direct touch necessarily precedes every other mode of examination. You feel, then, for the central point—the os uteri. In retroflexion or retroversion of the unimpregnated uterus this is generally found a little lower in the pelvis than is normal, and nearer the centre. In the natural state of slight anteversion the os uteri is directed backwards; in the abnormal state of retroflexion the os usually points downwards, perhaps a little forwards. Feeling the os, you of course ascertain its form and the condition of the intra-vaginal portion of the cervix. Associated with congenital retroflexion of the uterus, the vaginal portion is often found conical, and the os small and round, barely admitting a sound. Where this complication exists, something more than restoration of the body of the uterus to the normal position will commonly be required. But this subject may be put aside for the present. Your next step is to explore the roof of the vagina *in front of the cervix uteri*. Here, if the uterus is in normal condition, you may feel the solid rounded mass of its body through the vaginal wall; and, by combining the intra-vaginal touch with the extra-abdominal

touch—that is, by pressing down with the other hand the abdominal wall above the symphysis pubis, so as to meet the finger inside—you may take note exactly of the position and size of the uterus between them; and if the uterus is not there, its absence will be made manifest.

You next proceed to explore in like manner the posterior vaginal *cul de sac*. If you feel a hard, even, rounded mass *behind the cervix* through the vaginal wall, tender to the touch, you have strong presumptive evidence, strengthened by the absence of the body of the uterus from its normal position in front of the cervix, that this mass is the retroflexed uterus.

Rectal examination affords additional evidence. The finger in the rectum will even travel round the outline of this post-cervical tumour to a greater extent than can the finger in the vagina, and thus help materially the diagnosis. When by long practice you have acquired skill, you may be quite satisfied with this evidence. But this *tactus eruditus* itself is not acquired without a course of cross-testing by other methods; and to arrive at complete demonstration of uterine flexions the sound is necessary. Before passing the sound you must carefully consider the probability of pregnancy. In the great majority of single persons, and of those who have lived a married life for some years without children, the probability is strongly in the negative. The presumption is also in the negative in those who, having had children, seek relief from the consequences of retroflexion. Among these consequences is frequently acquired sterility. Two things help in forming an opinion: the date of the last menstruation, and the bulk of the uterus. If the bulk be sensibly increased and a period have been missed, of course you will not use the sound. At the same time you will remember that increased bulk of the uterus is an almost constant attendant upon retroflexion. To pass the sound you give the end of it such a curve as the idea you form of the degree of flexion of the uterus may indicate. For, example, if you feel the fundus of the uterus falling below the level of the cervix, and feel an acute angle of flexion, the curve given to the sound must be considerable. But in the majority of instances a moderate curve will be enough. And by a little manœuvre—that is, by lifting up the fundus of the uterus by your finger whilst the sound is passing the os uteri internum, the seat of chief flexion—you straighten the whole organ somewhat, and thus wonderfully facilitate the passage of the sound. The first stage of the introduction of the sound is best effected by passing the end into the os externum, as far as the os internum, with the concavity of the curve directed forwards; then turn the point backwards, so as to bring the concavity backwards, whilst the guiding finger lifts up the fundus. At the

same time you must carry the handle towards the symphysis pubis.

When the sound has gone the normal length of two inches and a half, as measured by the protuberance, you may very gently—lest adhesions oppose—ascertain the mobility of the uterus by bringing the point forwards again, so as to place the uterus in anteversion. Your finger behind the cervix now loses the firm rounded body, which, carried forwards, may be felt by the hand outside pressed down upon the symphysis. On withdrawing the sound, the tumour behind the cervix is reproduced, that is, the uterus has fallen back again. Then the demonstration is complete.

It was at one time supposed that this restitution or *redressement* of the uterus by the sound, if repeated frequently, would cure the retroflexion; but experience has proved this to be a delusion. Remove the sound, and the uterus commonly falls back again. Lift up the uterus gradually, as the lever pessary will do, extend the support it requires over a long space of time, and the structural change which is necessary for permanent cure will be effected. The action and mode of applying the lever-pessary we will next discuss.

Selection of a proper Instrument.—In choosing a lever-pessary we must bear in mind the principle of its action. It is a lever, and we must take care to use it as a lever. It is very easy to impair or to destroy this, its essential character, by using too large a pessary. This is the case when the instrument puts the vagina tightly on the stretch. It then acts very much like the old ring-pessary, which prevents the uterus from falling chiefly by expanding the vagina and resting upon the floor of the pelvis. The lever-pessary requires no bearing upon the solid structures of the floor of the pelvis, and ought not to stretch the vagina. A lever must be mobile. It depends for its efficacy upon its responding to the natural movements of respiration. At every inspiration the diaphragm, descending, drives the abdominal viscera downwards, and the anterior wall of the vagina, with the cervix uteri and the base of the bladder closely adherent to it, descends. Now, if a bar or lever be so placed in the vagina that one end rests upon the anterior wall, whilst the other is applied to the upper part of the posterior wall, so as to impinge upon the depressed fundus of the uterus, it is obvious that with every inspiration the anterior arm of the lever, carried down by the descending anterior wall of the vagina, will cause the posterior arm to ascend, and thus lift up the fundus uteri. This is precisely how the lever-pessary works. Its action is in strict harmony with the physiological movements of the organs. It is gentle, gradual, and safe. It preserves the natural contractile condition of the vagina. And

this gradual action is the great condition of cure of a retroflexion. At the seat of flexion, if this has existed long, there is change of structure—perhaps some degree of atrophy. Before the uterus can maintain itself erect, this spot of impaired structure must be restored to its normal integrity. This is a process of nutrition for which time is required; hence the lever-pessary must usually be worn for a considerable time, counted by months—perhaps for a year or more; but all the while it is doing good.

The material of the Pessary.—The best is undoubtedly vulcanite, on account of its perfect smoothness, and inalterability under exposure to the heat and discharges of the vagina. It is, therefore, perfectly clean; and when you have determined, by observation, the suitable size and shape, it is well to have one made in vulcanite for continuous use. But in the first instance it is more convenient to use the ordinary gutta-percha ring mounted on copper wire; this you can fashion yourself to the proper shape.

The size is determined by the capacity of the vagina and vulva, and by the degree of descent of the uterus. In single women one of very small size is commonly best; the elasticity and firm apposition of the walls of the vagina maintain the pessary in position. The touch gives you an estimate of the capacity and tonicity of the vagina. A ring $1\frac{1}{2}$ in, or 2 in., or $2\frac{1}{4}$ in. in diameter is generally large enough. In married women, especially in those who have borne children, a ring of $2\frac{1}{2}$ in., 3 in., or $3\frac{1}{2}$ in., may be necessary. It is imporant that both ends should have but a moderate curve—should be, in fact, nearly straight. This enables the lower end to adapt itself better to the symphysis pubis and urethra, and the upper one to the rounded uterus. The lower arm is bent backwards, the upper one forwards.

The introduction is effected in this way. The patient lies on her left side. The forefinger of the left hand is first passed as a guide just inside the vulva, and presses the perineum slightly backwards. The pessary held in the right hand is then passed with its flat in a line with the vulva—that is, in the conjugate diameter of the pelvis. As soon as the upper end enters the vulva it is directed somewhat backwards, so as to avoid pressing against the symphysis. When it is wholly inside the vagina the guiding finger applied to the lower arch turn the pessary half round on its long axis, so as to bring its flat into the transverse diameter of the pelvis. The effect of this manœuvre is often to carry the upper arch in front of the os uteri. The guiding finger, therefore, passed inside this arch must press it back under the os, when it will immediately rise to its proper position behind the os and below the retroflexed body of the uterus.

The riding of the pessary over the os sometimes causes a little pain. *In situ*, the os falls inside the upper loop, and displacement, provided the size and form are right, is nearly impossible. The adaptation of the lever depends upon the structure of the vagina—upon its anterior wall being shorter than the posterior; so that the anterior arm of the lever must be lower than the posterior one, which naturally rises into the *cul de sac* behind the cervix, to which place it is guided by the cushion between the lower part of the posterior wall of the vagina and the rectum, which forms an inclined plane rising towards the fundus of the vagina. The last thing to do is gently to push up the lower or anterior arch behind the symphysis with the tip of the finger. In single women the passage of the vulva is generally painful, and sometimes the displacement of the uterus is attended by such a severe degree of hyperæsthesia that the use of chloroform is indicated alike to facilitate diagnosis and the application of the instrument.

When *in situ* a well-chosen pessary should give no pain. If the vaginal wall is found much stretched, the pessary should be removed and a smaller one tried. Pain sometimes is felt from the pressure of the upper arch upon the tender congested uterus. Where there is any serious amount of inflammation, some days' rest in bed, perhaps weeks, should be used first. But generally the little uneasiness at first experienced soon wears off, and, as the pessary begins to act, sensible relief from the troubles due to the retroflexion is obtained. The pressure upon the bowel is gradually taken off, and after a time, aided by tonics and other means, the constipation is often materially lessened.

To remove the instrument, manœuvres in the inverse order of those used for introduction are necessary. There is no occasion, however, to bring the upper loop first in front of the os uteri. All you have to do is to hook the forefinger into the lower loop, which is directly behind the symphysis pubis, and whilst drawing it downwards and backwards to rotate it on its long axis, so as to bring the flat into correspondence with the vulvar fissure, and then to extract in the axis of the pelvic outlet.

Some few patients acquire the knack of introducing and removing the instrument themselves, but generally it is an operation that requires skill. It is worn continuously. It interferes with no function. Moderate exercise is beneficial, both by improving nutrition and general health, and, also by promoting the specific lever-action of the pessary. It is always desirable to see patients who are wearing the pessary from time to time—that is, once in two or three months. After a while, as the uterus rises, it may be useful to substitute a larger instrument. When endocervicitis or uterine catarrh or hemor-

rhage, complicates the displacement, as is not uncommonly the case, further local and general treatment will be necessary to accelerate the cure.—*Medical Times and Gazette, July* 29 *and August* 12, 1871, *pp.* 121, 181.

81.—ON POLYPUS OF THE UTERUS.

By Dr. J. Matthews Duncan, Edinburgh.

[Ordinary polypi are of three different kinds, fibrous, fibrinous, and mucous. Mucous polypi are the most frequent. There is no symptom of the disease worthy of description. No doubt various kinds of pain or uneasiness may be caused by them, but there is nothing distinctive in these sensations.]

The great sign of polypus is that loss of blood which is the main source of the importance of the disease. This hemorrhage may be menorrhagic—that is, occurring at the menstrual periods; but it may be incessant, or it may be accidental, occurring at any time. It is also highly important to remark that a polypus may exist without any hemorrhage whatever, and even grow while there is suppression of the menses, or while the menstrual flow is gradually getting scantier. I have never seen a false fibrous polypus, or a fibrinous polypus, which was not at some time or other the cause of bloody discharge. But the frequency, and therefore the importance, of bloody discharge or hemorrhage has been generally very much exaggerated by describers. Among the forty-one mucous polypi to which I have already referred, nineteen, or about a half, caused no extraordinary loss of blood whatever; while in several there was less than ordinary loss, and in some none of any kind.

It is rare to find considerable loss of blood produced by a small polypus, but I have observed it. The largest losses dangerous to life, which I have seen, were produced by true fibrous polypi, or by false fibrous polypi, which were undergoing extrusion from the uterine wall. In only one of the twenty-two fibrous polypi was there no extraordinary loss of blood.

When a mucous polypus causes serious loss of blood or anæmia, it does so, not by a flooding or in a short time, but by constant, or nearly constant, slight oozing. Of course, to produce anæmia, the oozing must be to an amount greater than the system can repair as fast as it flows. On the other hand, bleeding from a fibrous polypus is generally more profuse; sometimes it deserves the name of a flooding. I have seen a woman's life in imminent danger from a single flooding in a case of true fibrous polypus, and it is not rare to observe cases

where two or three hemorrhages have produced the most aggravated anæmia and all its painful consequences. Mucous polypi comparatively seldom, and comparatively slowly, produce extreme anæmia. Among the sixty-six cases to which I have been referring as mostly occurring recently in my practice, none proved fatal. In more than one it was my opinion that life could not have been much prolonged if an operation had not been performed. My impression, founded on experience, is, that death from bleeding in a case of polypus is much more uncommon than in a case of ordinary fibrous tumour.

Besides blood, mucous or muco-purulent fluid is frequently discharged from the vagina in cases of polypus. Such discharge may be of the nature of a simple vaginal or cervical uterine leucorrhœa, or it may be produced by superficial ulceration of the polypus or of the cervix uteri.

In cases of fibrous polypi, whether true or false, there is frequently a more or less copious watery discharge, of a more irritating kind than the ordinary leucorrhœal discharge, and sometimes to such an extent as to be weakening.

A woman can never be said to have a polypus unless the practitioner has either felt or seen it; in most cases he can easily both see and feel it. A mucous polypus is generally soft and rounded, but it may be dense and have various shapes. To the eye it generally presents a darker red tint than the neighbouring parts, as also does a fibrous polypus. Sometimes a true fibrous polypus is hard and nodulated, and of a very pale or yellowish white colour.

A false fibrous polypus is hard, nodulated, and very pale in colour, except occasionally, when it is much soiled by long lying in the genital passages. A true fibrous polypus is not always hard. When it is œdematous, it may be soft, and even deceive the practitioner, causing him to believe that he has to do with a bag of fluid. A fibrinous polypus may be broken down by the finger, disrupting its outer fibrinous wall.

Polypi certainly have no sensitiveness, and at least generally no sensation.

When a practitioner suspects the existence of polypus, and yet, on a vaginal examination, does not find it, he has to consider whether he will further pursue the search for it. If this further search is merely carried the length of dilating the external os uteri, the practitioner may proceed without any anxiety. But if he proposes to dilate the whole length of the cervix for the passage of his examining finger, he must proceed with the greatest circumspection. For the exploratory operation may itself be injurious, even more so than the disease which is as yet only suspected to exist. Metritis, ovaritis,

perimetritis, parametritis, are frequent results of this exploration, and death itself may be a consequence. I entertain a belief—of which, however, I have no sufficient evidence—that the forcible dilatation of the cervix uteri, in cases whose upshot shows that it was required, is not nearly so liable to be followed by untoward accidents as when it is done in cases which turn out not to have required it. Similar statements have been frequently made as to other dangerous remedies, and a so-called law of toleration, founded on such facts or fancies, has been enunciated.

It is very rarely, indeed, that dilatation of the whole cervix is required. In only one of the sixty-six cases was it resorted to in order to discover the disease.

Dilatation is effected by means of a tangle-tent. Sometimes a small one may be first used to begin the dilatation; and after it has done all in its power, or in about twenty-four hours, it is replaced by a larger; and again, by one even still larger. The tent, with a string attached to its lower end, is seized by a vaginal forceps, or throat forceps, and passed into the uterus. It is well to introduce a small sponge into the vagina to keep it *in situ*. It is removed by pulling the string attached to its lower end. After the removal of the tent, the index-finger of one hand is passed into and through the cervix, while the other hand, pressing on the abdomen above the pubes, pushes the womb downwards upon the examining finger.

Besides this kind of exploration, very little is required in the way of diagnosing a polypus. A cauliflower excrescence has a rough surface, friable texture, a broad attachment, constant fleshwater discharge, and free bleeding when handled; characters which a polypus does not present. Chronic inversion of the uterus—a very rare affection—may cause a little more difficulty than a cauliflower excrescence. It is to be distinguished by the following points: its history, its occasional sensitiveness, its regularity of form, the largeness of the part which may be mistaken for a pedicle, the absence of the body of the uterus in its natural situation, and the impossibility of passing far up a probe at any part between the cervix uteri and the suspected pedicle. But it is a general rule that a complication or complications are the cause of extraordinary difficulty in diagnosis; and the practitioner must remember that a polypus may complicate an inversion of the uterus.

There is nothing simpler than to tell what should be done in a case of polypus. It should be removed.

In most cases the operation is very easy. A small mucous polypus may be removed by torsion. It, or its pedicle, is seized by an appropriate long forceps, and, by twisting, removed or divided. This little operation may be done with the aid of a

speculum or without it. When the polypus is not very small, exceeding in size the phalanx of a small finger, it is best to clip through the pedicle of the polypus by an appropriate curved scissors. When the polypus is not large, and the woman has borne a child, the duck-bill speculum may be used to expose the polypus with a view to the operation; but it is generally convenient to dispense with a speculum of any kind. The operation may be done without an assistant, but it is better to have one. The operator seizes the polypus with a volsella in order to fix it, and in order in some cases also to pull it nearer the os vaginæ. The operator then guides an appropriate curved scissors by the fingers of one hand to the stalk of the polypus. His other hand holding the scissors makes it grasp and cut the pedicle. A pretty long curved scissors is generally used, but most such instruments are badly made, the cutting parts being too long and weak, in consequence of which they yield and disappoint the operator by refusing to cut. This error, however, is easily corrected by any cutler.

Instruments have been invented for cutting through the pedicle of a polypus, but they are illustrations of misplaced ingenuity. There is no difficulty in cutting through the pedicle of any polypus, and nothing is better for the purpose than a bistoury or an appropriate scissors. There is occasionally difficulty in reaching the stalk of a polypus, but no instrument for dividing the stalk will facilitate this part of the operation. A bistoury or a scissors will reach the stalk, if it be accessible, better than any other instrument.

The operator, in the case of a large polypus, may find the operation of removal very difficult. The difficulty lies, not in dividing the pedicle, but in getting sufficient access to it. In the case of an enormous polypus, where the difficulty is greatest, the division of the pedicle, if it could be effected as a preliminary to the operation of removal of the polypus, would make little difference in the difficulty of doing so. In a case of this kind, the difficulty is almost exactly that of delivering a fœtal head through a contracted outlet or over a rigid perineum. In both cases the perineum is likely to be torn, and sometimes the laceration extends even through the sphincter ani.

In cases of false fibrous polypi, the operator not unfrequently finds it unnecessary formally to divide the pedicle at all. Having fixed one or two volsellæ in the tumour to pull it through the os vaginæ, he finds, when the birth of the tumour is effected, that the false pedicle is also divided. I have never seen this happen with the true pedicle of a true fibrous polypus. On examining a false fibrous polypus after it has been removed in the way described, it is easy to distinguish the recently sepa-

rated connexions from the rest of the surface of the tumour which has been for some time lying bare in the genital passages.

The ordinary operation, in the case of a large polypus, consists in fixing one or more volsellæ in the polypus, dragging it forwards through the orifice of the vagina, and then cutting through the pedicle as soon as it can be reached. If the pedicle be short, this dragging down involves considerable displacement of the uterus; but really the displacement is not very great, if the shortest route for the cervix uteri—that is, following the direction of the anterior wall of the vagina—is pursued. The stalk of a polypus does not require to be cut off; it shrinks and soon disappears.

As already said, the difficulty of operating for an enormous polypus is sometimes very great, and the difficulty consists in getting it out of the vagina. The getting access to the pedicle is a matter of comparatively little importance. Yet when the pedicle is reached all the difficulties of the operation are past, not because the pedicle is reached, but because the bulk of the tumour is extracted from the vagina. Many plans have been proposed for facilitating the extraction—such as cutting out a wedge-shaped portion of the accessible part of the tumour, or cutting the tumour spirally, as it is called. But these plans have an appearance of precision which I have been unable to imitate. I proceed on a simpler plan, removing portion after portion of the tumour, and this, if possible, in such a way as always to leave a convenient bit of the tumour projecting from the remaining part, by which piece a good hold is got for pulling the tumour further down. So, bit by bit, the whole is removed.

A fibrinous polypus may come away spontaneously; or it may be detached and removed by the finger, used in a hook-like manner, as the obstetrician frequently employs it to remove early abortions; or it may be broken up and pulled away by a forceps.

After removing any kind of polypus, no bleeding is to be expected. In a case of small mucous polypus, or false fibrous polypus, or fibrinous polypus. I have never seen bleeding follow the operation of removal. After the numerous operations for polypi which I have performed, including those of the sixty-six cases referred to in this paper, I have not once seen an alarming hemorrhage, and I make it a rule not to plug the vagina after the operation, or do anything to restrain, except keeping the patient in the horizontal position. But I have seen a few times—certainly not five—an amount of bleeding which rendered hæmostatic measures expedient. In all of them, the bleeding began at the time of the operation, except in one case of fibrous polypus, where it was delayed till a few hours after

the operation. In a case of mucous polypus where bleeding occurred, the pulsation of an artery in the stalk could be easily felt, and the large vessel was subsequently seen in the removed portion of the pedicle. In all of the cases a very gentle plugging of the vagina was sufficient to arrest the hemorrhage.

Fear of hemorrhage after the operation, and anticipated difficulty in severing the pedicle, have been for a long time, and to a great extent still are, bugbears of gynæcologists, and have led to considerable sacrifice of life from their inducing operators to resort to bad methods. I lately removed a large true fibrous polypus in the Royal Infirmary from a patient who, immediately after the operation, insisted upon returning home to a great distance by railway, and who never had any further trouble or confinement from the disease. This simple operation was witnessed by a stranger, who had recently seen a similar operation in a large hospital done by aid of Gooch's canula. The woman was a fortnight in bed, and at one time her life was considered to be in danger. The contrast of the results of these cases is not only fair, but might be made infinitely more striking by taking, instead of the second case, one of the numerous cases where strangling the neck of the polypus has proved fatal to the woman. A great variety of instruments has been invented for the purpose. I recommend the disuse of them all. The procedure is slow and clumsy, and should be rejected on these accounts, even were there no danger to life from the sloughing and other processes consequent on the strangulation of the pedicle.

It is scarcely necessary to say anything of the palliative treatment of hemorrhage in a case of polypus, for this is a temporizing which, under any combination of circumstances, can scarcely ever be required. Such palliative treatment I shall dismiss with a very few words; for it is the same as is adopted in menorrhagia, and it is equally efficient—a statement which, however ironical, is yet true. He would be a bold physician who would dare in any case to predict that any palliative would have the desired effect. The mere number and variety of such hæmostatic remedies, recommended as they are in menorrhagia, and in the bleedings of fibrous tumours, is to the thoughtful physician good presumptive evidence that they are all pretty inefficient. Practitioners delight in them, and in changing from one to another. The patients are probably at least as credulous as the physicians, and are comforted.

I have never used in a case of polypus the local application of solution of perchloride of iron. But this, if required, would probably be as efficient as in the ordinary alarming bleeding from a fibroid.—*Edin. Med. Journal, July*, 1871, *p.* 1.

82.—ON FIBROUS TUMOURS OF THE UTERUS.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, and Obstetric Physician to the Adelaide Hospital, Dublin.

[A fibrous tumour may be defined to be a growth composed of fibrous tissue identical in structure with that of the uterine wall, but disconnected from it, being in general surrounded by a capsule of dense fibro-cellular tissue, which is perfectly dry and loose, so that when one cuts on the tumour it almost of itself escapes from its cavity.]

Fibrous tumours are met with of all sizes, from that of a grain of shot upwards; those of large size being by no means of unfrequent occurrence, while cases are on record in which they have attained a size greater than that of the uterus at the full term of pregnancy and a weight of 70 lbs. or even more. Again they may be solitary, but usually two or more are present in the same patient. They may spring from the peritoneal surface of the uterus and can be felt through the abdominal wall; they may grow from the submucous tissue of the uterus; or finally be developed within the walls of the organ: consequently fibrous tumours are spoken of as belonging to one of the three classes—namely sub-peritoneal, sub-mucous, and intra-mural, according as they are found to grow in one or other of the situations I have designated. Of these the extra-uterine or sub-peritoneal being entirely beyond the reach of treatment must be dismissed after a brief notice. They vary in size and appearance in even a greater degree than either of the other varieties, sometimes being numerous, small in size, and sessile, giving the surface of the uterus a nodulated appearance, or on the other hand attached by a pedicle which is sometimes short and thick, or at other times so long and slender as to permit the tumour to float as it were free in the abdominal cavity, and finally even to dissever itself from all connection with the womb, and possibly to become attached to some other portion of the peritoneal surface. When sub-peritoneal fibroids are pedunculated they sometimes descend into the pelvis, and then by their pressure on the neighbouring organs give rise to most distressing symptoms. When this occurs the patient's sufferings are sometimes very severe, incessant desire to micturate or total inability to pass water being frequently experienced. Of course it is impossible to give relief unless the tumour be raised from its position and replaced above the brim. This is always a matter of great difficulty, sometimes an impossibility. The tumour invariably lies in the posterior *cul de sac* between the rectum and uterus, occupying much the same position which the impregnated uterus does when retroverted. With the view of

raising it above the brim Dr. Kidd has adapted to this case the method suggested by the late Dr. Halpin, of Cavan, for restoring the uterus when retroverted during pregnancy to its normal position. He introduces one of Barnes's largest-sized India rubber bags into the rectum and gradually distends it with water by means of a syringe, while at the same time steady pressure is made with the finger on the tumour through the vaginal wall. In this way you will often succeed in raising the tumour and making it slip up into the false pelvis. Unless indeed the case be of long standing, and it be bound down by adhesions, should these exist your efforts will be not only useless but injurious.

Sub-peritoneal fibrous tumours do not necessarily give origin to menorrhagia, indeed as a rule they do not seem to influence menstruation at all. These tumours also generally spring from the posterior surface of the uterus; this however is far from being always so.

The sub-mucous pedunculated fibrous tumour is, prior to its removal, in no way distinguishable from, and is to be treated in a manner identical with the ordinary fibrous polypus of which I have already spoken. I shall not therefore allude to it any further but shall proceed to the consideration of the third and most important variety of these tumours.

Intra-mural or as they are sometimes termed the parietal or interstitial fibrous tumours are of frequent occurrence. They differ from the sub-peritoneal in two important features—namely, that they nearly always cause menorrhagia, and that they nearly as invariably stimulate the uterus to enlarge, an effect not often produced by the other form. Thus in Dr. Morgan's case just alluded to, though the tumour weighed upwards of 11 lbs. and was at least 25 inches in circumference, the uterus was of nearly its normal size and shape, while the presence of even a very small intra-mural tumour has been known so to stimulate the womb that it has grown to a length of five or six inches, while its walls have attained a thickness of an inch or more. Dr. West in his work "On Diseases of Women" mentions a case illustrating this fact. The growth of an intra-mural fibrous tumour is sometimes very slow. In a case at present under my observation, and in which the womb has attained a length of five inches, no appreciable change has taken place during a period of two years. On the other hand the tumour sometimes steadily increases in size, and then one of three results must occur—either it will bulge out the peritoneal surface of the uterus and possibly may become a sub-peritoneal tumour, or it may continue to grow in the substance of the uterus, the whole of the organ enlarging as the tumour increases, or it may project into the uterine cavity

carrying before it a covering of the muscular tissue of that organ. It is easy to conceive how this process if continued may result in the formation of an intra-uterine tumour, connected with the wall by a pedicle consisting of muscular tissue continuous with that of the uterus, and of the mucous membrane covering it. That this pedicle may in time elongate, and as it lengthens become more slender, till finally it passes out of the uterus, or even spontaneously breaking its attachment is expelled from the vagina, nearly all writers with the exception of Dr. Matthews Duncan admit the possibility of this process. He however thinks that the uterine wall never elongates before the true intra-mural tumour, but that the tumour is expelled *bare* into the uterine cavity, enucleation of the tumour, a process to which I shall have to refer by and by, having taken place spontaneously. However one thing is quite certain, that these growths frequently present themselves as well-defined tumours projecting into the cavity of the uterus.

Very frequently, however, fibrous tumours appear as mere protuberances bulging out the uterine wall. Such tumours as these can hardly be removed with an *écraseur*, and yet you cannot leave them alone, for health is undermined, and life itself frequently endangered, by the hemorrhage arising from their presence. The treatment to be adopted in such cases necessarily divides itself into the palliative and the radial; the former consists of restraining the profuse flow which occurs at each menstrual period, by plugging the vagina, as recommended in a former lecture, and by the administration of hæmostatics, such as gallic acid, alum, &c., while ergot alone, or in combination with the perchloride of iron, is often useful. But this plan of treatment is most irksome to the patient, and can only be looked on as a means of delaying the fatal results which ere long must follow if more energetic means be not adopted.

Medicines without number have been administered with the view of causing the absorption of fibrous tumours of the womb. Prominent among these are the bromides. I have tried them fully and freely, and believe them to be of very little, if any, use. It would be waste of time for me to go through the long list of drugs which have been recommended in these cases. I do not wish to deter you from trying them in your future practice; they will probably do no harm, but I think I can promise that they will effect little good. For myself, I have lost all faith in the resolvent powers of medicines of this class in the disease at present under consideration.

The very limited good produced by medicines has induced obstetric surgeons to adopt energetic treatment, no less than five methods having been recommended and practised with the

view to the radical cure of these embedded fibrous tumours. They are—1st, Incising the cervix uteri; 2nd, Incising the tumour; 3rd, Cutting into the tumour and destroying a portion of its tissue, a process to which the term gouging has been applied; 4th, Enucleation of the tumour; 5th, Avulsion or the forcible tearing away of the tumour from its attachment. Incising the os was first practised in this city by Dr. McClintock. This operation has been founded on the theory which, according to Mr. Baker Brown, is "that the division of the os and cervix uteri permits the fibres of the body of the uterus to contract upon the contained tumour, and thereby to compress the vessels and prevent hemorrhage." Whether this be the true explanation or not, one thing is quite certain, that the operation is often followed by good results, and in the case of very large tumours which are contained within the uterus, and when the cervix is thinned and spread over them, the operation is fully justified. The incising of the tumour has been practised by Dr. Attlee, in America, by Dr. Tracy, of Melbourne, and others, with success, a success which is probably due to the fact that the vitality of these tumours is nearly, if not altogether, destroyed by the incisions having divided their capsules, for the fibrous growth itself is endowed with but a very low degree of vitality. I have not met with a suitable case in which to try this treatment, but I certainly should not hesitate to do so were dangerous hemorrhages to occur in a patient in whom an intra-mural tumour existed, which I could not control by other means.

Enucleation, that is the cutting down on and dividing the capsule, and then grasping the tumour and turning it out of its capsule, is an operation suggested by a consideration of one of the processes by which nature occasionally effects a spontaneous cure, in which the capsule and investing covering of the tumour becoming thinned at one point, either by a process of absorption or ulceration, the contained tumour is then pushed out by the contractile power of the uterus, and is finally expelled. Enucleation is advocated by Dr. Matthews Duncan with his usual ability. He also practises with great success, the operation of avulsion, that is the seizing of a tumour with a strong vulsellum, and forcibly dragging it from its attachments.

Avulsion is practised by Dr. Duncan in cases in which spontaneous enucleation has been already partially begun, or where that stage having been artificially commenced, has proceeded to a certain extent. He considers it to be the proper practice in those cases of fibrous tumours in which the patient's life is in great danger, which medical treatment is unable to avert. I am not able to speak from personal experience as to

the value of the operation, but you will find full details of Dr. Duncan's views on the subject in the twelfth volume of the *Edinburgh Medical Journal.* I am equally without experience as to the merits or demerits of "goughing," but am of opinion that surgical means have been carried rather too far in the treatment of some of these fibrous tumours.

There is a less heroic mode of treatment I would have you bear in mind, and, under certain circumstances, to practise before having recourse to surgical measures. It is the injection, after previous dilatation, of tincture of iodine or of the liquor of the perchloride of iron into the uterine cavity. This practice is warmly advocated by Dr. Routh, of London, and if the cervix and os internum *be first dilated,* so that the injection may have a free and rapid exit, I do not think that it is likely to be followed by unpleasant symptoms. My friend, Dr. McClintock, informs me that he has recently injected tincture of iodine with marked success in the case of a lady whom I had an opportuning of seeing with him, and in whom alarmingly profuse menstruation occurred from time to time, and which he ascertained to be dependent on the presence of a large fibroid.

Dr. Matthews Duncan has recorded two cases in which he successfully restrained dangerous hemorrhage depending on the existence of a tumour in the uterus by the injection of the liquor ferri. perchloridi, one drachm of the fluid being in each case injected by means of a hollow sound into the cavity of the womb. In his cases the cervix does not seem to have been dilated, a precaution I should always adopt.

I have now given you an outline of the pathology and treatment of the various forms of fibrous tumours, but there yet remain two interesting and important phases of their history, to which I must allude before concluding the subject; the one, the increase and subsequent decrease in their size, which is sometimes observed, the other their occasional absorption, transformation, or even elimination.

All fibrous tumours, especially the sub-mucous, when they hang into the cavity of the uterus, are liable to become œdematous, and to this cause many of the recorded cases of enlargement and subsequent decrease in their size is referable. But in addition to this cause, menstruation and pregnancy undoubtedly influence both the condition and size of these growths. In many cases a fibrous tumour which ordinarily is productive of no discomfort to the patient, becomes at each menstrual period the seat of pain. This is a fact I have several times noticed. That actual increase in bulk should also occur at the epoch is easily understood. The following case illustrating the fact is recorded by Dr. Ernest Lambert, of

Paris:—"Age of patient, 38. For ten years past a tumour appeared before each menstrual epoch, disappearing in turn to reappear again; for a year it has ceased to disappear, and has become the seat of severe pain." After death a large fibrous tumour was found growing from the anterior surface of the uterus. From the same author I here quote the following instructive case on the authority of M. Depaul, who relates that having been summoned to a patient, at a distance from Paris, he found three physicians in attendance on a primipara, supposed to be three months pregnant. She had suffered for some time past great difficulty both in passing water and in defecation, and for four days previous to M. Depaul seeing her had been unable to empty either the bladder or rectum, even the catheter could not be passed except with great difficulty. She suffered from the most powerful expulsive pains, and her agony was very great. M. Depaul recognised the existence of a large fibrous tumour which filled the pelvis; the patient's state was one of great danger. With difficulty he reached the os uteri, introduced a sound, and brought on premature labour. The next day a fœtus, flattened like a sheet of cardboard, was expelled; in a short time this tumour had decreased to a third of its former size, and at the end of four months was not larger than a small apple; it was situated in the anterior wall of the uterus, near the neck.—*Medical Press and Circular, Aug.* 16, 1871, *p.* 131.

83.—CASES OF CANCER OF THE WOMB SUCCESSFULLY TREATED BY BROMINE.

By Dr. A. Wynn Williams, Physician to the Samaritan Hospital, and Physician-Accoucheur to the Western General Dispensary.

[The details of the following cases contain full particulars as to the mode of treatment.]

Case 1.—*Medullary Cancer.*—Mrs. P., æt. 30, mother of one child, consulted me May 16th, 1868; well fed, but of leucophlegmatic temperament. She complained of pain across the loins, extending down the thighs, causing a constant feeling of sickness; bowels much constipated, always acting with pain and difficulty; menstrual discharge regular but very profuse, lasting seven or eight days; constant leucorrheal discharge. Tongue furred, heart's action feeble.

On making an examination per vaginam the finger came upon an uterus lower than natural, with a protrusion of the posterior lip to the size of a walnut, hard and smooth, not very painful on pressure; anterior lip and body, although large,

appeared healthy and readily moveable. On examining with the speculum, it was with considerable difficulty, although using a full-sized one, that I could get the posterior lip within the end of the speculum; when within there was seen a mass about the size of a walnut partially smeared over with a creamy-looking discharge; when this was wiped off with cotton wool, the tumour was seen to be glassy and white, with a few dark spots scattered over the surface; anterior lip swollen, with open os. The appearance indicating the stage shortly preceding the softening and ulceration of a malignant tumour (medullary carcinoma). She was ordered an aperient and to take a mixture containing tinct. ferri perchloridi, tinct. nucis vomicæ, and spt. chloroformi. I determined to inject the growth with a solution of bromine, which I did at her own house on the 19th, using the syringe handed round, made of glass and platinum, charging the syringe with a solution of bromine, bromi. min. xij ad spt. vini rect. ʒ j. I passed the syringe through the speculum down to the growth, which, however, I found so hard I could not get the point to penetrate, until I procured a pair of hooked forceps, with which I took hold of the neck, drew the womb forward, whilst I forced the syringe inwards with a kind of half screw motion to nearly half an inch. I, before forcing out the fluid into the tumour, covered the surface of the exposed parts with cotton wool saturated with a solution of carbonate of soda, and then withdrawing the syringe a few lines so as to free the point, forced the contents about twenty minims into the tumour, leaving the syringe in *sitû* about two minutes. On the withdrawal of the point of the syringe a few drops of the fluid followed, but was immediately neutralised by the soda. As the patient complained of a good deal of pain, a quarter of a grain of acetate of morphia was injected under the skin of the arm, which soon sent her off into a doze.

May 19th. On examination the surface of the tumour, to about the size of sixpence, was seen to be of an ashy colour, but the slough had scarcely begun to separate. She had not remained in bed, and was tolerably comfortable. Ordered to continue the mixture and to use an injection, by means of an india-rubber syringe, of bromine min. xij, spt. vini. rect. ʒ ij, aquæ dest. ℥ xvj, three times a day.

23rd. The slough had not quite separated, but it could be seen to what extent the tumour had been destroyed, nearly one-half consisting of the anterior portion. I determined not to wait for the entire separation, but proceeded to inject the other portion of the tumour, which I did by passing the point of the syringe through the opening already made and forcing it backwards and upwards, taking care not to pene-

trate the mucous membrane covering the back of the tumour, using the same precautions as on the former occasion. Some of the bromine solution on pressing down the piston was forced through into the cavity already made. On removing the cotton wool the raw surface was seen as it were scorched. The patient complained of a good deal of burning pain, which was again relieved by the subcutaneous injection of morphia. She was also ordered ext. cannabis indicæ gr. i, each night, if required.

29th. The slough caused by the first injection had now separated, the other separating; any hard or suspicious-looking portion was touched with the bromine solution held in *sitû* for a few minutes. She had not had occasion to take the pills. To continue remedies.

June 5th. All the sloughs had now separated, leaving a large cavity. As there was still some suspicious-looking growth at the back part, it was again touched with the bromine solution.

12th. The cavity was filled with healthy granulations, there being no appearance of disease. To continue the bromine injection and mixture.

19th. Parts skinning over. From this time the wound rapidly healed and attained apparently a perfectly healthy condition. The patient visited me again in the May following, when I examined and found the parts quite healthy. I have not seen her professionally since, but have met her walking about apparently in perfect health.

Case 2.—*Medullary Cancer.*—Mrs. L., æt. 28, widow, mother of one child, consulted me May 7th, 1868. Had never enjoyed good health since her confinement nine years previous. Five weeks after confinement suffered from severe pain in the left side; six weeks after there was a great discharge of matter from the vagina. Has since then suffered more or less from pain and a discharge. Consulted me more particularly as to the propriety of again getting married. Catamenia irregular, not profuse. Pain in back and loins running up to left side under the ribs. On examination the womb was felt low down in the vagina, moveable, with anterior and posterior lips large, hard, and protruding. Examined with the speculum the os was seen stretched out laterally with everted edges. Sides higher up, being pressed closely together by a tumour on either lip, the one on the anterior lip being the larger and of a more pearly appearance. Before proceeding to destroy the tumours by bromine I had recourse to leeching and other remedies, usually employed for the removal of chronic enlargement of the os and cervix, without avail; and as matters were certainly getting worse instead of better, I determined on June 17th to attempt the removal of the growth in the anterior lip by

bromine. This I proceeded to do by applying over it cotton wool saturated with bromine solution (♏xij ad ʒ j), by means of a vulcanite cup, taking the usual precautions to prevent injury to the adjacent parts. The bromine was kept applied for two hours at my own house, the patient residing some miles off. On the removal of the cup, &c., the tumour was seen to be whitened as if scorched. The patient complained of a good deal of pain during the application, but was able to proceed home in a cab. She was ordered to inject a weak bromine lotion, and to take a mixture with tinct. ferri perchloridi, &c., and also, if required, every night ext. cannabis indicæ, gr. j.

June 23rd. Slough not yet separated, which appeared to include nearly the whole of the tumour. To continue remedies.

26th. Slough not entirely separated.

30th. More than two-thirds of the tumour removed, but the base of the tumour was unduly hard. A piece of cotton wool was now dipped in the strong solution, and kept applied to the wound for about ten minutes, the evaporation of the bromine being prevented by covering the wool, &c., with gutta-percha sheeting. On removal the wound was observed to be well scorched. The bromine and spirit was again applied on July 3rd, 7th, and 10th; on the 15th all the granulations appeared healthy, and the wound was allowed to heal, the injection of the bromine lotion being continued.

July 18th. Wound healing.

22nd. Nearly healed. Informed my patient I should proceed to destroy the other growth on the following visit, but she never appeared, and I saw nothing of her for twelve months, when, on the 31st of July, 1869, she again paid me a visit. She informed me that family matters had necessitated her instant departure from London, and that she had not had time to communicate with me before leaving. She also informed me that she had got married, but had suffered from wretched health ever since I had last seen her, indeed, had been too ill to return to London. She had been suffering from an attack of ague and rheumatic fever, for which she had been attended by a medical man in the country, but never mentioned her uterine ailments. She had suffered much from pain in the back, loins, &c., and irregular menstruation, sometimes very profuse, at other times very slight, and coming on at irregular intervals. After recruiting her health by rest, nourishing diet, tonics, &c., I proceeded on September 1st to destroy the tumour in the posterior lip, which had considerably enlarged during the last twelve months, being also of a more pearly colour, with here and there a few dark spots. There was also considerable creamy discharge. The same means were adopted to destroy this tumour as had been used on the former occasion.

The patient was allowed to leave town apparently quite well on the 21st of October. I have not heard of or from her since. I must just remark that there was no appearance of the return of the disease in the anterior lip.

Case 3.—*Epithelial Cancer.*—Mrs. B., æt. 49, presented herself before me at the Samaritan Hospital, February 13th, 1867. Mother of several children; had ceased to menstruate. Complained of pain in the back, &c., and great irritation in the genitals caused by the acrid discharge. On examination there was found sprouting out from the side of the os uteri a bright red growth, broader at the extremity than at the base, the extremity spreading out exactly like a branch broken from a cauliflower, bleeding readily when touched, and having a velvety feel. I diagnosed it to be an epithelial cancer in its commencement. To it the strong bromine solution was applied by means of cotton wool, &c., and was left in contact for about half an hour. She was also ordered to inject the weak bromine lotion. The same process was gone through on the 19th and 26th. On March the 5th there was none of the outward growth remaining, but the depression or stump, so to speak, was very red and hard. A bit of cotton wool rolled round the point of a stick wetted with the bromine solution was inserted into the depression, and held there for twenty minutes. This procedure was repeated on the 12th and 19th. The strength of the lotion was now reduced by one-half, and she was ordered ol. morrhuæ ʒ ij twice a day.

March 26th. The hole was now about an inch deep, with no appearance of disease. She was ordered to inject a simple lead lotion, and by May the 9th she was to all appearance quite well. Dismissed, with strict injunction to show herself from time to time, which she did up to the end of June. On the 25th the spot from whence the growth had been removed felt hard and looked very red. She was ordered liq. arsenicalis, min. v, twice a day in water, and to inject the bromine lotion.

July 9th. Tumour evidently growing again. I now determined to inject the strong solution with the syringe, and forcing the point of the syringe through the centre of the growth to the depth of two-thirds of an inch, forced in as much of the fluid as I could. To continue the arsenical solution and the injection, and to take pulv. ipecac. comp. gr. x each night if in pain.

19th. The growth was again injected, the syringe being passed through the opening already formed to the depth of fully an inch.

30th. The hole formed by the separation of the slough was nearly an inch and a half deep, and as the bottom felt to the probe somewhat hard, the bromine solution was applied by

means of cotton wool, and again on August 6th. The arsenical solution was now discontinued, as it caused griping. After this date it was not considered necessary to apply any more of the strong solution, and the wound was allowed to granulate, using only the bromine lotion. I must here remark that the solution had never penetrated the canal, and that the growth appeared to have had its origin in the vicinity of the inner os. The patient continued under observation for some weeks after the wound had healed. I also saw her several times the year following, and up to the present time I believe there has been no return of the disease.

Case 4.—*Epithelial cancer.*—Mrs. M., æt. 33, presented herself at the out-patient department of the Samaritan Hospital, February 11th, 1868. Had been married twice and had seven children; age of youngest four years. Was delivered of a dead child last November; present husband a sweep. Has been an inmate and out-patient at several hospitals, where, as she states, she was treated for consumption, the state of the womb never having been examined. She complained of pain in the left side which went through to the back, also pain in the lower part of the abdomen, thighs, back, &c., and a great discharge, very acrid and somewhat offensive. Catamenia natural. A tumour could be felt on the anterior surface of the neck of the womb, commencing near the os and extending upwards as far as the reflexion of the mucous membrane to the vagina, which felt rough and velvety, not painful to the touch, but bleeding rather readily when handled. The womb was somewhat enlarged but moveable. On looking through the speculum a growth very much resembling sweep's cancer, as seen on the scrotum, was observed, only of a more vivid red colour. The surface was not much elevated, and was of greater extent than the base, as a probe could be readily passed under its several lines. The vagina having been protected with soda, &c., the strong bromine solution was placed over the surface of the growth, and covered over with gutta-percha tissue, and fixed by filling the speculum with cotton wool, the whole being left there about an hour. On removal the growth was seen to be whitened over its whole extent. She was ordered to take a chalybeate mixture, and inject the bromine lotion three times a day. By the 21st the slough had separated, when the strong bromine solution was again applied in the same manner.

April 25th. The surface of the tumour having been nearly all destroyed, the base was now injected with the strong solution. As the lotion caused a good deal of smarting, it was ordered to be discontinued and warm water to be substituted.

Feb. 28th. Had suffered a good deal of pain, bowels constipated, was ordered a pill containing ext. cannabis indicæ, gr. i, ex. colocynth co. gr. ii, every night.

March 6th. Slough caused by injection not yet separated.

15th. Slough separated, suspicious-looking parts touched with the strong solution.

19th. Touched again, and ordered to resume the injection of the lotion. It is unnecessary to follow the particulars of the case further. I need only remark that under the use of the lotion the wound filled up, and the patient got quite well, and in the following year, having removed to the country, was delivered of a living child, which died some months after birth. She is now again near her confinement, if she has not already been confined. She has had no return of the disease.

It must, I think, be admitted that cancer of the womb, when it comes sufficiently under the cognizance of the physician, is amenable to treatment, or at any rate certain forms of it. My own opinion is most decided that cancerous growths or formations situated in the lips or neck of the womb can be successfully eradicated when confined to this part only, and previous to the commencement of ulceration. The fact of ulceration having set in must not be considered by any means as precluding any attempt being made for the removal of the disease; on the contrary, as long as the womb is moveable and the glands and other parts apparently healthy, the fact of ulceration having commenced should be an incitement to immediate action. In point of fact, the earlier the disease is discovered, and active measures taken, the better are the chances of the patient's recovery. No evil can possibly accrue, even supposing the tumour operated on should not be malignant, as it is certainly as ready a mode of getting rid of it as any other with which I am acquainted. The manner in which the disease should be dealt with will necessarily vary in different cases. The more superficial, as some of the epithelial varieties, will be best destroyed by saturating cotton wool with a strong spirituous solution of bromine, placing over it a vulcanite cup or gutta percha; any stump left must be treated as the more solid tumours, and injected. The solid fibroid growths are best treated by injection of their substance, and for this purpose a small trochar and canula, to which a glass syringe can be attached, answers best. It is not easy to force the point of a syringe into the more solid growths, and, further, it is by no means easy to force the fluid into the tumour. Without freeing the point of the syringe all attempts to force out the contents will be futile. To do this the syringe must be withdrawn a line or two. On the other hand, with a fine trochar these difficulties do not occur, the steel point is readily forced in, and, when withdrawn, leaves a space beyond the end of the canula.

The strength of the solution I now invariably use as an

escharotic is twelve drops of bromine to a drachm of rectified spirit of wine. This proportion I find to act far more powerfully than does pure bromine. It is not necessary to mix it immediately before use. I find that when it has been mixed even weeks and months its escharotic properties seem to be just as powerful as they were when the solution was first made, even though it may have almost lost its colour. This is no small boon to the operator, as the smell and irritation caused by the escape of the fumes of bromine from the recent solution is most distressing, and, at the same time, very injurious. The fumes of bromine have so weakened the delicate structure of my olfactory nerves, that I have been for many months without the sense of smell; it is now returning, though very slowly. Before manipulating with bromine I now take the precaution of moistening some cotton wool with a solution of soda, and placing some of it in each nostril. I am not at all satisfied in my own mind as to what is the nature of the change that takes place when bromine is mixed with rectified spirit of wine. There is effervescence with evolution of heat. After a time the liquid, although stoppered ever so carefully and kept in the dark, gets paler and paler, but the loss of strength by no means keeps pace with the loss of colour. Probably we have bromide of ethyl and hydrobromic acid formed. On making trial with pure hydrobromic acid, I was suprised to find it very feebly escharotic. It is advisable that the solution should always contain some free bromine. I must not forget to mention that the injection of the bromine lotion into the vagina is a very essential part of the treatment; I find the surface of the wound under its use always clean and having no offensive smell. It was by observing the beneficial effects in open cancers in other parts that I was induced to employ it in diseases of the womb. I am, as many of you are aware, a great advocate for the application of weak solutions of iodine to scrofulous ulcers, and in this way I was led to try the effects of bromine on cancerous sores. This happened some twelve or fourteen years ago, when practising in the country. After leaving the country on account of ill health, several years elapsed before I had an opportunity of carrying on further experiments. It was not until I became attached to the Western General Dispensary that such an opportunity occurred, and it was at this institution that I first injected a spiritous solution of bromine into cancerous tumours.

Some of the results of this plan of treatment you have heard; whether the plan can be advantageously adopted when cancer affects other parts is still to be proved. I have never been able to give it what I consider a fair trial, the cases have been all too far advanced; nevertheless, the results have not

been void of promise. The internal exhibition of bromine has not been sufficiently tried. I have ordered it on one or two occasions, but only for a short time; its use, to be of benefit, would, I should suppose, require a long continuance. The most convenient form of administering it is to mix it with glycerine and water; thus mixed it is not unpleasant to take—one patient considered the taste not unlike raspberry vinegar.

[The above paper, which was read before the Obstetrical Society, was freely criticised, especially by Dr. Playfair, who remarked that Dr. Williams's cases afforded no conclusive proof that they were malignant at all, inasmuch as fixature of the womb, which is the first positive sign of malignancy, did not exist, indeed, in each instance the womb was said to be freely moveable. Dr. Williams stated in reply, that there must be a time in the early stages of the disease when it was confined solely to the neck of the uterus, and in which the womb was not fixed; such was the case in the mammæ and other organs.]—*Transactions of the Obstetrical Society of London, Vol* xii., 1871, *p.* 294.

84.—ON TUMOURS OF THE BREAST.

By Christopher Heath, Esq., Surgeon to the Hospital for Women, and Assistant-Surgeon to University College Hospital.

[Even the highest authorities are not agreed as to whether undoubted cancer is a local manifestation of a constitutional taint, or whether the disease is at first purely local and the constitutional affection a consequence of the local mischief. The word "cancer" should be reserved for those tumours which, in addition to the naked-eye and microscopic appearances generally recognised as characteristic of that disease, have a clinical history showing progressive infection of the lymphatic system, leading to secondary deposits and a constitutional cachexia.]

The *recurrent fibroid* or *fibro-nucleated tumour*—the chief characteristics of which are a close external resemblance to the ordinary fibrous tumour, but a tendency to recurrence after removal, with a progressive softening of successive growths—is occasionally found in the breast, and an excellent example of it has recently been put on record by Mr Nunn. (*Path. Soc. Trans.*, vols. xviii. and xix.) The tumour was developed in the right mamma and measured 23½ inches in circumference, weighing 4 lb. 12 oz. after removal. The patient was twenty-eight years old and the mother of two children; and the tumour had grown

in two years, ulcerating through the skin at the centre of the growth fourteen months after it was first perceived. There was no enlargement of the lymphatic glands. The tumour was removed in March, 1867; and the patient made a good recovery. The tumour was lobulated and its structure fibrous, with spindle-shaped cells in abundance. Towards the end of May the patient discovered a fresh outgrowth; and on her readmission to hospital in July a mass the size of two fists was to be seen at the site of the original tumour. On July 31st Mr. Nunn repeated the operation, but it was evident at the time that the disease had extended through the intercostal spaces and could not be entirely removed. Fresh sprouting of the growth occurred, and the patient died exhausted on October 23rd of the same year. At the post-mortem examination, no secondary deposit in internal organs was found, but the tumour was seen to have invaded the pleural cavity, forming rounded protuberances underneath the costal pleura. Microscopic examination showed the tumour to be composed of filamentous tissue abounding with nuclei, and such as would be generally classed as a fibro-nucleated tumour.

The experience of these recurrent tumours in other situations besides the breast is conclusive as to the necessity for complete and early removal, and therefore nothing less than the removal of the entire breast should be thought of when any recurrence of an apparently simple tumour takes place. Even then the prospects of the patient are as gloomy as in true cancerous disease; for the growth is almost certain to reappear, and to destroy the patient, either by the exhaustion of repeated operations or by involving the pleural cavity.

The *myeloid tumour*, whose characteristic is the large multinucleated cells resembling those of the fœtal marrow, is found much more frequently in connection with bones (and especially the jaw-bones) than in the mamma. Doubts have been thrown upon the return of myeloid tumours; but, from what I have seen in the case of the jaws, I have no doubt that myeloid tumours are frequently, though not constantly, reproduced. I have never met with a case in the breast, and must therefore content myself with referring to one recorded by Mr. Paget. The patient was fifty years of age, and had an irregular tumour, between two and three inches in diameter, in her left breast, which had existed for nine months. On removal, it proved to be myeloid; and eighteen months later the patient returned with a large ulcerated tumour in the corresponding axilla, from which she died.

The forms of cancer occurring in the breast are scirrhus, medullary, and colloid; and their frequency agrees with the order in which I have placed them, scirrhus being more com-

mon than medullary disease, and colloid cancer being of rare occurrence.

Scirrhus or *hard cancer* is rarely seen before thirty years of age, and may appear at any period after that, but about fifty is the most common age for the disease to show itself. Unmarried women appear less liable to the disease than marrie- women. Thus, of 260 cases of scirrhus given by Mr. M. Baker, 23 per cent. were in single women, 72 per cent. in married women, and 4 per cent. in widows; and consequently, as Mr. Paget remarks, the percentage among single women is smaller than in the female population generally. Commencing as a small nodule, so painless at first that it is often overlooked for some time, the disease slowly increases in the majority of cases, though occasionally, in what has been termed acute scirrhus, the increase is rapid. Soon, as a rule, pain is experienced in the tumour, of that peculiar lancinating or stabbing character which is often looked upon as pathognomonic; but it must be remembered that absence of pain is by no means conclusive as to the non-malignancy of the tumour, since some cases of scirrhus run their course with little, if any, suffering, As the tumour increases, the tendency to involve and drag upon the surrounding tissues is developed: and thus, if the disease is central, the nipple becomes retracted and fixed; or if peripheral, the skin overlying it is bound down and dimpled. Lymphatic enlargement now takes place, the glands along the axillary border of the pectoral muscle being those ordinarily affected, though the glands above the clavicle are also liable to infiltration, particularly when, as Sir Astley Cooper has remarked, the original disease is on the sternal side of the nipple, so that the internal mammary lymphatics become the means of conveyance of the morbid material.

Ulceration of the retracted skin is the next pathological feature, and there is, I think, more variation in this than in any other part of the disease. In some cases the ulceration is so slow, and the consequent discharge so slight, that the patient is able, for months and even years, to conceal her malady from her nearest relatives and friends; whilst in others the ulceration proceeds with rapidity, and extends either widely or deeply —for it rarely does both, The depth to which scirrhus may penetrate is of course limited only by the pleural cavity, perforation of which has been often witnessed; but to the extent to which scirrhous ulceration of the skin may reach there is really no limit save the vital powers of the patient. Thus, a fortnight since, I saw a woman, in very fair general health, with an enormous ulcerated surface on the chest and axilla of the size of a plate, which showed no tendency to become arrested but by the death of the patient.

By the time the skin over a scirrhous breast has become at all extensively ulcerated there will in most cases probably be some amount of constitutional cachexia produced, particularly if enlargement of the lymphatic glands has been an early symptom in the case. Added to this, we have the œdematous and painful condition of the arm due to obstruction of the axillary vein by the cancerous deposit; and the patient is at last worn out with the pain and suffering acting upon a lowered vitality. The duration of a case is affected very much by the age and constitution of the patient, its course being slower in the older and less plethoric individual; but the dictum of Sir Astley Cooper may be considered a fair one, taking the average of cases—namely, two years for the full development of a scirrhous breast, and from six months to two years longer for a fatal termination to the case. Instead, however, of running its ordinary course, the disease may become arrested in its progress by a form of atrophy, and the patient may then live for years, and die from disease unconnected with the breast. It is, so far as I have seen, in thin elderly women that this atrophic scirrhus is found; and it may happen, as in a case recently under my care, that the disease will suddenly light up afresh, as it were, and its renewed activity will require immediate operative interference.

In these cases, and also in other cases of scirrhus, particularly (in my experience) those in which the primary disease has been removed with success and without return in the breast, we meet with growths of cancer in internal organs—such as the liver, stomach, or uterus,—which may be taken as evidences of a constitutional taint consequent upon or antecedent to the primary disease, according to the pathological creed of the surgeon. Development of cancer in an internal organ is, however, ordinarily so much less painful a cause of death than open cancer of the breast, that this well-known liability to the development of internal cancer ought to form no bar to operative interference in suitable cases. In addition, moreover, to these well-recognised positions of cancer, I have once seen a patient, whose breast had been removed some months before, covered all over with small cancerous tubercles of the skin, varying in size from a pin's head to a medium-sized button; and as there was ptosis with strabismus on one side, together with symptoms of brain disturbance, I have no doubt that a cancerous tumour was developed in the brain which pressed upon the third nerve, though I was unable to procure a post-mortem examination.

Medullary cancer is much more rapid in its course than scirrhus, often destroying life in a few months. As might be expected from the differences in structure of the two growths,

the medullary tumour is larger, softer, and more succulent than the scirrhous cancer. Its tendency to infiltrate the neighbouring tissues is much greater; and hence, when the skin gives way, it is by a process of sloughing, or very rapid ulceration, leaving a chasm through which protrude masses of soft bleeding cancer, giving the appearance characterised as true *fungus hœmatodes*. Any rapidly-increasing solid tumour of the breast must be viewed with suspicion, and more particularly if the veins running over it are much engorged, and if there is enlargement of the neighbouring lymphatic glands. In this form of disease there is much greater probability of a development of secondary tumours in other parts of the body than in scirrhus; thus not unfrequently patients suffering from medullary breast are carried off by some acute chest attack, when secondary deposits are commonly found in the pleura and lungs.

Colloid cancer is exceedingly difficult of recognition prior to removal; in fact, almost all the examples of it have been mistaken for some other disease. The disease wants the hardness which is so characteristic of scirrhus, and is less rapid in its growth than medullary cancer, the lymphatics being also more slowly affected. A good example of colloid tumour of the breast, with a secondary tumour in the axilla, of five years' growth, is recorded by the late Mr. Price in the 8th volume of the Pathological Society's Transactions, and has appended to it an elaborate report upon the microscopic structure of the growth, by Dr. Andrew Clark.

A section of a recent scirrhous tumour shows a dense white structure, the fibrous nature of which is at once obvious to the naked eye. On scraping it a whitish juice exudes, and this under the microscope shows an abundance of cells, with nucleus and nucleolus, which may be considered to be the so-called cancer-cell. I have the advantage of showing you Mr. Arnott's beautiful drawing of the microscopic appearance of scirrhus, and I cannot do better than quote his own description. The drawing shows "the typical form of hard cancer—*i.e.*, cells of an epithelial type, of varying size and shape, but with tolerably uniform (and usually single) large nuclei closely packed in the meshes of a stout fibrillated stroma, without any visible cellular elements." The proportion of cells to the fibrous stroma it is that determines the character of the growth, for if the cell element preponderates over the fibrous, we have the more vascular form of medullary cancer or encephaloma; whilst if the cells have degenerated, and the fibrous interspaces are filled with a little fatty fluid only, we have the atrophic scirrhus.

There is, then, no special microscopic appearance by which medullary cancer can be distinguished from scirrhus, except

by considering it as a whole, and here the naked-eye appearances and clinical history assist us much. The brain-like texture, the great vascularity, and the rapid growth and invasion of surrounding structures, enable us to recognise encephaloma in almost all cases. It is remarkable, as showing how one form of cancer runs into the other, that the secondary deposits in the axillary glands and internal organs after scirrhus of the breast partake more of the medullary or soft form of cancer than the hard; and, in fact, Mr. Arnott's illustration, which I show you, is taken from an axillary gland. This fact is not so surprising, however, when we find that, as the recent researches of MM. Cornil and Ranvier have shown (by means of nitrate of silver as a reagent), the lymphatics communicate directly with the interspaces in the fibrous stroma of scirrhus in which the cells are lodged. It is not surprising, therefore, that the cell element should predominate in the secondary deposits thus produced.

The structure of colloid cancer is unmistakable, a section showing the loculi filled with gelatinous material of various shades of colour which are so characteristic of the disease. Microscopic examination shows a delicate fibrous stroma, enclosing in its meshes cells, of which some are round or oval, mononucleated, and having within the outer cell-wall several very delicate concentric lines, giving to the cell somewhat of an oyster-shell appearance (H. Arnott.) Other large polynucleated cells are found, and also an arrangement of the stroma in concentric circles, enclosing a number of nucleated cells, and having nuclei interspersed among the concentric layers of tissue.

Few persons in the present day would deny the hereditary nature of cancer, though the late Mr. Charles Moore held that the disease "should rather be styled heritable than hereditary," since he believed that the failures in transmission were more numerous than the occurrences.

The results of operations for cancer of the breast vary of course considerably according to the nature of the case and the previous health of the patient. The operation itself is a remarkably successful one as regards immediate danger to life, although every surgeon must occasionally lose a patient after this, as after every other surgical proceeding, even the simplest. But, in stating the risk to a patient, one has the satisfaction of pointing out that in all probability great relief from immediate suffering will be obtained, and a possible immunity from return in favourable cases. With regard to the prolongation of life as influenced by an operation, professional opinion has undergone a change within the last few years. It was thought by many surgeons that, though comfort was gained by an operation, yet that life was shortened; but the elaborate tables of Mr. Sibley

and Mr. Baker, published in the *Medico-Chirurgical Transactions*, show a different result. Mr. Sibley, whose statistics are drawn from the records of the Middlesex Hospital, states that "in the cases of cancer of the breast, those who had been operated on lived 53 months, whilst those in whom the disease was allowed to run its natural course lived only 32 months." Mr. Baker, whose data are drawn from Mr. Paget's experience, says "the average length of life in scirrhous cancer is 43 months when the primary disease is not removed, and 55 months when the operation is performed; whilst in the case of medullary cancer the results are even more striking, being 20 months without, and 44 months, or more than twice the time, with an operation." These results are sufficiently encouraging, and probably the statistics of operation cases may prove more and more satisfactory as both the profession and the public become convinced of the necessity for an early and complete removal of cancerous growths.

With regard to the average date of recurrence of cancer, the greater malignancy of the medullary cancer is again seen, that disease ordinarily recurring within one year, whilst scirrhus does not reappear till the second year, the average times given by Mr. Baker being nearly 14 months for scirrhus, and 7 months for medullary disease.—*Lancet, June* 24, 1871, *p.* 847.

85.—A NEW CAUSTIC FOR MOTHERS' MARKS.—CARBOLIC ACID.

By Dr. George Berwick, Sunderland.

It may not be uninteresting to some of your readers to call their attention to the following case which occurred lately in my own practice.

Mrs. W., a young lady, during an early stage of her last pregnancy, received a strong mental impression by accidentally seeing a young girl with a large hairy mark on her face. The consequence was, that her own child, when born, was marked in a similar way. One half of the nose, the upper part of the left cheek, and over the left eyebrow half way on the forehead, there was a large hairy, corrugated, dark-coloured, ugly-looking mark. As an experiment, I tried the strong glacial carbolic acid with the most satisfactory result. I took four ounces of carbolic acid, B. P. (as manufactured by Davy, Yates, and Routledge), put the bottle in warm water till the acid liquefied, and then added a teaspoonful of distilled water, in order to keep it from again crystallising. With this liquid acid I painted the mark twice a day, taking only a limited portion of the mark at one time. By repeated applications, a

scab or crust formed, which ultimately scaled off, and left the skin beneath perfectly free from blemish, much to the mother's gratification. I assure you the whole of the large mass is now removed, with the exception of a small portion about the size of a half-crown, part of which is over the upper eye-lid, which we are now touching more cautiously and slowly in order to save the eye from harm.—*Lancet, Sept.* 2, 1871, *p.* 347.

86.—ON THE TREATMENT OF INCESSANT HICCUP.

By Dr. John Rose, Chesterfield.

[The following admirable epitome of the treatment of hiccup was elicited by a letter of enquiry on the subject to the Editor of the *Lancet.*]

Allow me to recommend the application of the ether spray to the neck and epigastrium (so as to affect the pneumogastric and phrenic nerves), with full doses of bromide of potassium, at the same time giving ice and soda-water. Should this fail, I should be inclined to blister the neck, and give large doses of quinine during the day, and chloral hydrate at bed-time. Dr. John Constable, about two years ago, related in your columns a case of persistent and alarming hiccup in pneumonia, which was cured by the subcutaneous injection of morphia under the intercostal muscles. This treatment is well worthy of further trial. Widal records a case of obstinate hiccup, accompanied by serious symptoms, which was cured by twelve-grain doses of quinine, repeated every day in the same dose for three days. The hiccup, which had lasted without intermission for nineteen days, never returned. The same author relates another case which was cured by valerianate of zinc with extract of belladonna, and a third by Vichy water and gentian. In the last case the stomach appeared to have been chronically inflamed. In continued fever, hiccup is occasionally troublesome, and will sometimes yield to small doses of rhubarb. Dr. Habershon related, about eight years ago, to the Medical Society of London two cases of hiccup. One could be restrained by counting continuously, and it stopped on the patient walking about. It was cured by iodide of potassium given freely, then steel and blistering to the neck. In the second case the hiccup preceded the formation of a perineal abscess, and did not finally cease until it had been opened and was quite healed. Dr. H. thought both cases due to some indirect irritation of the pneumogastric nerve acting on the spine, and thence reflecting on the phrenic nerve. In the case of a young woman in whom hiccup was present night and day, depending on some intestinal irritation, the late Dr. Todd gave a scruple of calomel, and recovery ensued. —*Lancet, July* 29, 1871, *p.* 181.

ADDENDA.

87.—ON THE PHYSIOLOGICAL ACTION OF THE ORGANIC HYDRIDES.

By Dr. Benjamin W. Richardson, F.R.S.

[When hydrogen combines with the organic radicals it forms hydrides of them. The hydrides of methyl, ethyl, propyl, and butyl, are highly inflammable gases, but the *hydride of amyl* is a liquid, so light that it boils vehemently in the hand. A glass globe of it boils actively by merely placing the palms of the hands on the sides of the globe. It is thin and bright, and runs like water. It is this liquid, which, diluted with ether, is known as anæsthetic ether, and as it is itself perfectly innocuous, and procurable in any quantity by careful distillation of American petroleum oils, we can hardly foresee the extent of its future utility in medicine. The following are some of the practical applications of Amyl Hydride.]

Anœsthetic Ether for Local Anœsthesia.—The first application studied had relation to the production of a new fluid compound for local anæsthesia by the spray process. The hydride having been found soluble, in all proportions, in rectified ether, various combinations of the two fluids were made, and the effects registered. In freezing with the spray, it is not the best practice to freeze too rapidly, for if the superficial parts be instantly frozen, the layer of frozen surface acts as a non-conductor, and deeper freezing is rendered impossible. At the same time, the practice is bad that delays the freezing process too long, since delay gives pain in the act of freezing, and pain during reaction. The point to find, therefore, was the correct medium or proportion between ether and the hydride for the end held in view. After many experiments and many applications for operation on the human subject, I find that a mixture of one part of the hydride to four parts of ether is the most effective and ready fluid for spray. I call the mixture "Compound anæsthetic ether for local anæsthesia." As a rule, this compound induces, in the form of spray, perfect insensibility of the skin in from ten to twenty seconds of time. It gives less pain than ether, when applied to a cut or open surface; and in operations upon the teeth it is much better in action than the best ether used

alone. It may be diffused into the mouth, as spray, with perfect safety, having no quality that needs to be dreaded. Once or twice during long operations on the mouth, and when the compound vapour given off from the spray was unavoidably inhaled freely, there was produced general insensibility, but this was rather favourable than otherwise to the operative procedure.

Solutions.—I have said that the hydride of amyl is a solvent of many medicinal substances ; and this fact has led me to use it in medicine, as a solvent, in various ways. I show you some of the compounds thus formed.

Iodized Hydride.—Iodine dissolves readily in amyl hydride, and produces, in the proportion of twenty grains to the ounce, a solution of great service in practice. When this solution is applied to the skin, the volatile hydride passes off at once as vapour, and leaves the iodine, in considerable quantity, behind, stranded on the part in most equal form of distribution. This application is of singular utility in cases of hard open sores, where it is desired to apply iodine evenly and deeply. Thus, in cases of open strumous glandular disease, the solution plays an important part as a means of cure, and the same in chronic indolent bubo. In bad sloughing fetid ulcerative and suppurative wounds, and in cancer, no solution is so simple, painless, and effective. In these last-named cases the iodine exerts more than a curative influence—it deodorises; it destroys decomposing organic products; it prevents the absorption of decomposed products, and protects against the secondary fever depending on such absorption. In applying the solution in the cases named, it may be gently poured over the part. There is necessity neither for cotton-wool nor for the brush.

From the iodized solution of the hydride, iodine itself may be inhaled with advantage in cases of ulcerated throat, and in cases of cavity of the lung. Indeed, whatever value in the treatment of phthisis and of bronchial phthisis there is in iodine, it is best obtained by the mode of administration now being described. Of course we have amongst us much difference of opinion as to the actual value of iodine inhalation, and I do not suggest this method in support of any one particular opinion. I hold my own view, and in favour of the practice in fitting cases, but I wish, for the moment, merely to describe a ready method of applying the practice, so that all who wish may adopt it. In using the twenty-grain solution for inhalation it is best to dilute it with more of the hydride until the vapour of the iodine given is scarcely at all irritating to the throat. The patient's own sensations on the matter are here the best guide, and with a very little instruction it is easy to secure that five grains of iodine shall be inhaled at one time.

There is not the least occasion for hurry, or for causing the slighest constriction of the fauces, or pain. I usually administer from a little funnel of parchment paper, holding in it some finely teased cotton-wool, on which I drop the solution. From this funnel the patient breathes, holding it a short distance from the nostrils and mouth, so as to allow the admission of plenty of fresh air.

The solution of iodine in amyl hydride has another application, adapted this time not to the sick person, but to the chamber of the sick. There is no agent at one and the same time so potent for purifying the air of the sick-room as iodine. I introduced it several years back for purifying the air of the room or ward in which sufferers from small-pox are lying; and from all parts of the world, but from India especially, I have received recognitions of the value of the practice. But there has always been some difficulty in carrying out the process. Diffusion by volatilisation of the metalloid itself from a chip-box covered with muslin—a method invented by that able surgeon, Mr. Hoffman, formerly of Margate—although it is in many cases most effective, is in most cases too slow, and if I may use such a term, too local; while the plan of driving off the iodine by heat from a porcelain or metal plate is not a plan to be safely entrusted to a nurse. But with the volatile iodide solution all difficulty subsides. We take a packet of ordinary filter paper, the paper being cut into pieces three inches in diameter—in fact, cut as it is sold from the chemical storehouses—and on to a packet of one or two dozen, or more, of such papers we pour the solution until all the pieces are fully saturated with it. Then the papers are allowed to dry; they dry very quickly, and are put into a box ready for use. We give a dozen or so of these papers to the nurse, and tell her to keep some of them exposed to the air in two or three places about the sick-room, so that the odour of iodine may be faintly recognisable through every part of the room; and this done, all is done for ordinary circumstances. To meet any unusual unpleasantness of the air, the nurse may take one or two of the papers and burn them like a taper or spill, when the deodorisation will be the more rapidly and determinately carried out.

In instances where a room or ward has been occupied by infectious cases, and it is required to purify quickly and effectively, the iodized hydride may be used in the form of spray. The spray-producer to be employed must be constructed of glass, as a metal spray-tube is injured by the solution. Siegle's simple tube answers for the purpose well. It is advisable before using the solution to have the room to be disinfected completely stripped of all furniture, the walls rubbed down,

and the floors well swept, scrubbed, and dried. Then, from different positions in the room, the iodized hydride should be distributed in spray. The solution containing twenty grains to the ounce is strong enough. The room should have all its windows and doors closed before the iodine is distributed, and the quantity of solution sprayed should be measured. Practically, I find that one ounce of the solution to four square feet of space is a good adjustment of quantity to space. After the iodine has been distributed from the spray-producer, the room should still be kept closed for twenty-four hours at least; during this time the iodine deposited, at first, in the finest layer on the floor, ceiling, and walls, slowly volatilises, and, coming into contact with the organic matter, destroys it rapidly. It is prudent not to take a light into the room after the distribution of the solution, until the windows and doors can be reopened, as the amyl hydride vapour easily takes fire.

The most persistent and offensive odours in rooms that have been occupied by the sick may, by this simple method, be more speedily purified than perhaps by any other known method. In asylum practice we get the most difficult of tasks of purification; for, from the bodies of the insane, organic compounds —probably of the sulphur class—diffuse and permeate everything, yielding the most offensive smells. As products of disease, these, perchance, have not a little to do with the unhealthy condition and physical derangement of the bodies from which they are emitted; and when they once fix in an apartment or room, they stay with a perseverance that is wonderful. I was consulted quite recently, at a house I visited, respecting a room of the house in which an epileptic man died, even months ago. This patient, during his fatal illness, suffered from profuse perspirations giving off the most offensive odours, and still in the room where he had lain, despite all efforts at cleansing it, there was distinct evidence of the odour. To remove this unpleasantness there is nothing approaches iodine, as asylum experience has proved, and the best way of applying the iodine here, again, is by the spray process described. But when the process is being carried out, it must be carried out thoroughly. If the room be opened too quickly, and air be admitted so as to create too speedy a diffusion of the iodine, the cure will only be temporary, and after a lapse of three or four weeks the odour will be once more distinguishable; for these organic odorous products, if they be not absolutely destroyed, release themselves in time from the destroyer, and, being less evanescent, proclaim that the victory over them is incomplete.

[The nature and some of the properties and uses of hydride of amyl are explained in the preceding part of this article. There are, besides the solution of iodine in hydride of amyl, two other solutions of simple substances in which the amyl hydride is the solvent.]

Solution of Oils and Fats in the Hydride.—Common oil mixes in the hydride of amyl freely, and stearine, spermaceti, and other similar fatty substances, readily go into solution in it. When a solution thus made is exposed to the air, the volatile hydride evaporates, and leaves the oily or fatty matter behind. By this simple means we are able to leave upon the skin an even layer of substances which effectually exclude the air, and which, if required, may act also as a means for diluting more active remedies. There is here in the hydride a solution of spermaceti and olive oil; the spermaceti, cut into fine shavings, is added until the fluid is saturated, and then a sixth part of oil is added. This solution is most useful in the treatment of burns. It is gently poured over the burnt surface, and as the evaporation of the hydride proceeds, the cooling that occurs is an immediate source of relief from pain. In time, the part is left covered with a pellicle or false skin of fatty substance, and the air is entirely excluded. If, when the surface of the body is freshly covered with the solution, a little cotton-wool is lightly placed upon it, the rapidity of evaporation is subdued, and the dressing is more effective. I met myself with the accident of a severe burn this winter, and treated the injured part precisely as I have stated, and with the best success. The relief from pain was all but instantaneous, and, by re-applying the solution over the cotton-wool, I kept the pain entirely in abeyance until the balance of the circulation was restored. I left the dressing on the abraded surface, retaining it with a light bandage, and when, a few days afterwards, I removed the cotton-wool and the layer of fatty matter, I found the healing perfected. It is important in using this solution to avoid bringing the flame of a candle or other light near to the part, the vapour that goes off being very inflammable.

The advantage of the hydride over ether and other volatile solvents of fats is, that by its presence it excites no pain. The oxides and the chlorides themselves create local pain on sensitive surfaces—this fluid does not. In some forms of skin affection, where there are symptoms of extreme vascularity, heat, and irritation, the application of a mixture of the hydride and olive oil is very soothing and useful, and I should think it might be of service in some ophthalmic cases; but on this point I have as yet had no experience.

A solution of wax, or of spermaceti, in the hydride may be

turned to much good account in bandaging. The bandage is well saturated with the solution, and is applied while in the moist condition. In a short time the bandage is left quite dry, and the part encircled by it is encased in a firm but flexible structure, which effectually excludes the air.

Ammoniated Hydride.—When a solution of ammonia is placed in contact with amyl hydride, bubbles of gas rise freely from the ammonia, and diffuse into the light hydride. After a time the hydride may be decanted off, when it will be found strongly impregnated with ammonia. The mixture, I think, is merely mechanical; at the same time it is very useful for several purposes. The solution may be diluted with more hydride, until the pungency of the ammonia is reduced, and then the vapour may be inhaled—a method of administering ammonia which in many cases would prove of advantage. In scarlet fever, for which disease ammonia is so excellent a remedy (I had almost said an antidote), we are often embarrassed, in cases where the patients are very young, because we cannot get the sufferers to swallow ammonia in solution. In such cases, then, we could with little difficulty introduce the remedy by the lungs, and so sustain the fluidity of the blood and keep up the action of the heart.

A solution of camphor in this ammoniated solution of the hydride is useful as a preservative of animal substances, and may be applied for the preservation of specimens of natural history. The specimen to be preserved is dipped in the solution, and is allowed to remain in it until it is quite saturated. Then it is removed, and the hydride is permitted to evaporate, when the structure is left charged with the camphor, and is retained in good condition for a long period of time. Half an ounce of camphor in sixteen ounces of the hydride forms a good compound for the purpose named.

Lastly, on this point, the simple ammoniated hydride serves well for keeping pathological preparations during considerable periods of time. A good many years ago I discovered that if a little common ammonia be put into a closed jar with a pathological specimen, the specimen retains its freshness for many weeks. I now use the solution before us in preference to the simple ammonia, and with still better results. Into a pint jar I place, with the specimen to be preserved, half an ounce of the ammoniated solution of the hydride, and, after the vapour is well diffused, I close the jar closely with a tightly-fitting stopper. The jar being placed in a cool place, there is not the least occasion to hurry over an examination, for the specimen will keep (even for microscopical research) three or four days in excellent condition.

Amyl Hydride as a General Anæsthetic.—To complete the research instituted as to the physiological action of amyl hydride, I have experimented with it as a general anæsthetic. There are many points in common between this substance and amylene (the anæsthetic introduced by the late Dr. Snow).

To test the properties of the hydride, its effects were first tested on pigeons, guinea-pigs, rabbits, and frogs. In all these researches the same plan was adopted. The animal in each case was placed in this glass chamber, which has a capacity of 1000 cubic inches. The arrangements here are such that every condition of a practical kind is at hand. A measured quantity of the vapour is gently diffused through the air of the chamber with hand-bellows: an aneroid barometer within the chamber gives the atmospheric pressure, a thermometer registers the temperature, while, by means of a warming apparatus beneath, the air can be sustained at a fixed temperature at all seasons of the year. The amount of vapour yielded at the temperature that exists during the time at which the experiment is carried out is calculated, and, by a simple contrivance, the animal is so introduced that no air is let in to interfere with the charge of vapour within the chamber at the moment of the introduction. Thus we have means for obtaining the facts we require to learn, in a steady and systematic manner.

In order to produce a decisive effect with the vapour of amyl hydride, the air in the chamber was charged with different percentages of the vapour at 65° Fahr., and some preliminary experiments were made, in which the special symptoms induced were not noticed in detail, the production of decided anæsthesia being only sought. After this the particular phenomena presented were carefully noted.

The research with hydride of amyl was commenced by narcotising pigeons. Observations on these animals are specially important, for, as they are extremely susceptible to the dangers of anæsthesia, it may be safely inferred that an anæsthetic which proves safe to them will prove safe to the human subject. Moreover, in the pigeon, even more than in man, vomiting and muscular excitability during the action of the narcotic are easily induced; and, again, in them, modification of the animal temperature is always strikingly marked. In a word, so close is the analogy of symptoms caused by an anæsthetic in a pigeon and in the human subject, the value of any anæsthetic may be safely determined by the effect produced on the pigeon; and no new anæsthetic agent ought ever to be administered to the human subject until the inferior animal named has been found in several instances to pass through the ordeal with perfect safety.

When, then, in the case of amyl hydride, a pigeon is exposed

to an atmosphere containing from thirty-five to forty per cent. of the vapour, the period required for the production of decided symptoms is under a minute, and within two minutes the insensibility is so profound that the animal must be removed into pure air. The insensibility is accompanied with slight muscular movement, and sometimes with a drawing back of the head; but the muscles speedily relax, and the sleep is calm, and attended with the deepest insensibility. On removal of the animal from the vapour, and on placing it in pure air, the recovery is very rapid—from one and a half to two minutes being sufficient to insure entire restoration to consciousness. The specific lightness of the vapour, and its insolubility in the blood, account for the briefness of the insensible condition it produces. The temperature of the body of the animal is but slightly modified; there is a reduction of temperature, but it amounts barely to the fourth of a degree on Fahrenheit's scale. In this particular the amyl hydride differs materially from chloroform and ether. Chloroform administered to the third degree of narcotism causes a reduction of from 6·5 to 6 degrees of temperature in pigeons, and ether causes a reduction of 4 degrees. Perhaps the shorter period of administration required to produce the deep insensibility when the hydride is the agent employed, may account for the difference observed. On guinea-pigs and rabbits the vapour of amyl hydride acts with the same rapidity as on pigeons. Recovery from the effects is also equally rapid, while the animal temperature in these animals undergoes no apppreciable decline. In these animals the muscular excitement is not developed, but in the rabbit there is noticed a rapidity of breathing during recovery which instantly ceases if a little of the vapour be re-administered.

In order to see the extreme effects of the amyl hydride, the vapour of it was made to kill;—the animal after being narcotised was, I mean, retained in the vapour until the evidences of life had ceased. Animals of the three classes named above were thus allowed to sleep to death. The process of death is gentle—a continuance, as it seems, of the deep sleep. The circulation and the respiration appear to cease simultaneously; but in one case I could hear the heart in motion a few seconds after the cessation of respiration. Towards the close of the life the temperature falls suddenly from 1° to 1½° Fahr., and the pupils dilate.

After death the heart is found fully charged with blood on both sides and in all the cavities. The arterial colour of the blood on the left side is deciedly darker than is natural, but the coagulation is not interfered with, and the corpuscles show no evidence of change.

The lungs after death are not found congested with blood,

as in death from agents which kill simply by causing asphyxia, neither are they found bloodless and blanched, as is so common after death by chloroform and some other volatile chlorides. They are left retaining a natural colour and character. The brain shows no visible sign of injury.

For a long time after death from amyl hydride the muscles of the body retain their irritability, and may be called into vigorous action by electrical excitation. I recollect no agent that destroys life and interferes at the same time so little with the muscular irritability. Let it be understood, however, that this retention of condition for muscular motion under stimulus extends only to the voluntary and the semi-voluntary muscles. In the midst of the life of the other muscles the involuntary heart is dead, and makes effective response to no stimulus, not even to electricity.

The influence of the vapour is slower on frogs than it is on warm-blooded animals; but after a little time frogs become so profoundly narcotised, it is hard to determine they are alive, and if the temperature be lowered they will lie in the vapour for two and three hours, and still, on removal, will recover. Thus, if a strong, healthy frog, narcotised deeply, be placed in a a shallow glass basin, with a little of the liquid hydride around the body, the evaporation of the liquid will keep up such a degree of cold that change into fatal death is suspended; and when the liquid has evaporated altogether, if the temperature of the air be kept at from 65° to 70° Fahr., and a little water of the temperature of 70° be occasionally supplied to the animal, it will recover when every indication of life is, to the naked eye, absent. In plain fact, it is difficult to say when death has actually occurred. The muscular irritability may positively be so suspended that it cannot be called forth by stimulus, and yet recovery may be the ultimate result.

After I had conducted these experiments, I ventured to inhale the vapour of amyl hydride myself to the production of unconsciousness. I placed four drachms, by measure, of the fluid in this vulcanite inhaler, pouring the liquid upon a double layer of domette, with which the inhaler is loosely lined. The vapour was to me agreeable to breathe; it caused none of the suffocating sensation experienced from the inhalation of vapour of ether, and none of the irritation produced by the vapour of chloroform; but it induced a sensation of gentle glow or warmth in the chest. After six deep inspirations of the vapour, I felt evidence of change in the cerebral circulation—viz., giddiness and inability to stand firmly, with the swaying movement, or sense of movement, common to the first stage of narcotism. Then, for a few seconds, I lost consciousness, but the inhaler being removed, I recovered very quickly, and in three

minutes was perfectly well. Neither nausea, chilliness, nor headache followed the administration.

On the whole, of the facts thus gleaned by experiment, I infer that the amyl hydride, in addition to its other useful applications, might be employed as a general anæsthetic for the prevention of pain in certain Surgical operations, such as extraction of teeth, where the operative proceedings are short—not exceeding, that is to say, two or three minutes in duration.

There is one other application of amyl hydride, interesting in a physiological as well as practical point of view, to which reference should be made. It becomes of service in studying the question of restoration of life after death by extreme cold. In some animals of cold blood—fishes and frogs—if their bodies be suddenly frozen, it matters not how extreme, how absolutely through the mass of the animal body the freezing may be, there will still often be recovery on thawing their bodies, if the process of thawing be not too rapidly carried out. We may therefore, taking one of these frozen amimals with the intention of restoring it, so utilise the fluid as to moderate the thawing at will. We place the frozen animal in a tall glass jar, and over it we pour some hydride of amyl. Then we warm gently the lower part of the jar; and as the liquid can never be made to exceed the temperature of 86° Fahr., if the evaporation be perfectly free, the animal thaws in it without any danger of what is called reaction, and recovery is the general fact.

I cannot do better in closing this part of the lecture than demonstrate the fact just named by an experiment. Here are some frozen fish that have by the frost been frozen in an aquarium; they have been retained in the frozen state in this room by a surrounding of ice and salt; and here also, are some frogs frozen so completely that they are, you will say, of stony hardness. I will pass some of these frozen animals round, but one of them—a frog—I will retain. I take this glass jar, an anatomical-preparation jar, fourteen inches long and two and a half inches in diameter; I place the animal in the jar, pour over it five or six fluid ounces of the light hydride, and apply a gentle warmth so as to raise the fluid to its maximum of 86° Fahr. There is not the least occasion now to do more than wait and watch. In a short time, as the muscles of the animal relax, there will be a sign of renewed respiratory action, in the escape of a bubble of gas from the mouth of the animal. As I am speaking of it the phenomenon occurs, although there is no apparent breathing movement. That escape of gas is of itself quite sufficient. I turn the fluid, together with the animal, into a large glass funnel, having a narrow escape-tube, so that the fluid can run away into a receiver below it, and the animal be left in the air. Soon the animal recom-

mences to breathe, and, as you see, in the freshness of its renewed life, it leaps out of the vessel. It has recovered without sustaining a trace of injury.—*Medical Times and Gazette, September* 23 *and* 30, 1871, *pp*. 374, 401.

88.—ON HYDRAMYL AS A GENERAL ANÆSTHETIC.

By Dr. Benjamin W. Richardson, F.R.S.

In the course of the present year, 1871, I have made a new investigation of the amyl hydride as an anæsthetic. I was led to this new, or rather renewed, research by some observations on the effect of the compound anæsthetic ether, when it was being used in the form of spray. I have stated that this compound is a mixture of amyl hydride and rectified ether; and the facts I observed in using it for producing local anæsthesia were, that when it was applied for operations on the nose and mouth the patients often passed into a temporary general insensibility whenever the operation was prolonged. In an operation for cleft palate in a youth, Mr. Christopher Heath being the operator, this general anæsthesia was twice fully developed during the operation. In a case where a small but tedious operation was carried out for removing a piece of dead alveolus, the same event occurred, and even in two cases of simple extraction of teeth it was repeated. The sleep in all these cases was so insensibly induced, was so gentle, so deep, and yet so temporary, it could not fail to attract my attention, and as it was clear the insensibility was due to the inhalation either of the vapour of the hydride or of the ether, I began a series of inquiries as to the part played by the hydride. The labour has been useful in that it has led to the application of hydramyl for producing general anæsthesia with great rapidity for short Surgical operations. To amyl hydride I have given, for the sake of brevity, the name of *hydramyl.* I am indebted for the suggestion of this abbreviation to Mr. Ernest Chapman.

In recommending the research, I obtained from Mr. Robbins a good specimen of the fluid hydramyl. It had a specific gravity of ·625, and it commenced to boil at 86° Fahr. (30° Cent.). It was nearly inodorous, but to some persons it gave the sense of a faint odour. When breathed it created no irritation, and I found, by breathing the vapour of it out of a bag charged with it to the extent of 60 per cent., that I was almost immediately rendered unconscious, the recovery from the inhalation being singularly rapid. I found, also, that by inhaling it again, as I had done originally, from the simple vulcanite inhaler, the same effects could be produced.

On June 6 I administered hydramyl for the first time for a short operation—viz., the extraction of a firm molar tooth from

the lower jaw, Mr. Peter Matthews being the operator. The patient was a woman 30 years of age. I poured into a small vulcanite inhaler two fluid drachms of the hydramyl, and, letting the patient hold the inhaler herself, asked her to take a few deep inspirations. She carried out the instructions readily; and at the end of twenty seconds, as there was distinct evidence of an effect, I removed the inhaler, and asked her to open her mouth. She complied, and Mr. Matthews immediately extracted the tooth. The whole proceeding was from twenty-five to thirty seconds. Within the minute the patient had recovered, and was talking to us consciously. She said, immediately after she commenced to inhale she felt as if she were passing into a natural sleep. She remembered being told to open her mouth, and said she obeyed "as well as she could," but she could recall nothing more relating to the operation; she felt nothing whatever of the extraction. In recovering she had neither vomiting nor nausea, although she had breakfasted a few minutes before; in brief, her recovery was as perfect as it was rapid.

The character of the anæsthetic sleep itself was most satisfactory. It was induced without a movement of any kind; the face retained its natural colour and expression; the pulse underwent no change whatever.

There was in the case this phenomenon, on which I must dwell:—The insensibility to pain occurred before the actual abolition of consciousness. I have observed, with Snow, precisely the same condition from amylene, and have made the fact matter of comment in my reports to the British Association for the Advancement of Science. I have observed the same phenomenon in experimenting with methylic ether, and I do not think there can be a more interesting subject of study.

The metaphysicians, in treating of conscious and unconscious states of mind, have long taught that there may be periods of consciousness with an absence of common sensibility, and the truth of this inference is now sustained by physical facts. In several cases where I administered methylic ether for removing pain in surgical operations, the patients, when quite insensible to pain, were so conscious they were able to obey every request asked of them, and in some instances were even anxious to reason, stating that they knew what was going on, and arguing that they were not ready for the operation because they were sure they should feel pain. Nevertheless, in this state of mental activity they were operated on, and afterwards, while remembering every incident, were firm in their assertion that they felt no pain whatever during the operation. One patient, who sat for the extraction of two teeth, selected the tooth to be first extracted, putting her finger to it, and afterwards re-arranging her position for the second removal. To

the looker-on it seemed, in fact, as though no change in her life had occurred, yet she affirmed that she was sensible of no pain whatever; and several other less striking, but hardly less singular, examples came before me. We may then, I think, fairly assume that in course of time we shall discover manageable and certain anæsthetic substances which will paralyse sensation only, leaving the muscular power unaltered, and the mental little disturbed; and we gather from this either that in the cerebral hemisphere there is some distinct and simple centre of common sensation which may be acted upon by certain agents without involving all the cerebral mass, or that the peripheral nervous matter may be influenced without involving the other portions of the nervous system. On the whole, I incline to the view that the action of those agents which destroy pain before they remove consciousness is primarily on the peripheral system; for we know, from the process of local anæsthesia, that it is easy to destroy sensation at the extremities without destroying or even interfering with consciousness; while those who have inhaled the vapours which destroy common sensation before interfering with the mental condition, invariably describe the experience of a numbness and insensibility in the extreme parts of the body.

I offer this theory, wishing, as on all occasions, to enforce the fact only on which it is based. I have said the fact is of importance physiologically; it is of equal importance practically. To have an instrument in our hands by which we can, at will, induce insensibility to pain, with or without destruction of consciousness, as the case before us may demand, means, when it is perfected, an advance second only to the discovery of general anæsthesia itself—a refinement of the art rivalling the art. There are many minor surgical operations for which consciousness need not be destroyed, although pain ought to be; there are other operations in which the consciousness of the person operated upon is of great service to the operator; and there is a third class of cases in which it is essential to suspend both sensation and consciousness.

What will not, then, be the perfection of our science and of the art that follows it, when we can so master pain at pleasure as to isolate the consciousness from the common sensation, benumb generally, and from within, the periphery of the nervous matter, leaving the grey external matter of the cerebrum undisturbed; or plunge, if need be, consciousness and sensation alike into forgetfulness? Yet this in process of time is certain to be accomplished. It is an advance practically founded at this moment; it waits only for more labour to be made manifest to the world by its daily application.

Returning to the details of practice, I next administered the

vapour of hydramyl on June 19 and 24 to three patients for the extraction of teeth, and in both cases with success; but I found a little difficulty, owing to the higher temperature of the air that then prevailed, in retaining the fluid in the inhaler, its lightness and low boiling-point causing it to evaporate with too great rapidity; in fact, a few breathings of the patient emptied the inhaler. I therefore proceeded to an endeavour to meet this difficulty by slightly weighting the fluid with a heavier body, but one having still a vapour density nearly the same and a low boiling-point.

Hydramyl-Chlor.—In making bichloride of methylene, we place a mixture of alcohol and chloroform in contact with pure zinc. On the application of heat there is set up a brisk action, during which an equivalent of chlorine from the chloroform ($CHCl_3$) passes to the zinc, and, after a free escape of gases, bichloride of methylene (CH_2Cl_2) distils over. On my request, Mr. Robbins, while manufacturing some bichloride of methylene, added to the materials in the retort prepared for making the bichloride about eight times the volume of amyl hydride. The result was an immediate brisk action without the aid of heat. A copious stream of gases first passed over, and then, the fluid in the receiver having risen in temperature, there began to distil a compound fluid, very light specificially, and of a most agreeable odour. Collecting some of this fluid, I found it had a specific gravity of ·699, and that it commenced to boil at 92° Fahr. It was much more manageable for inhalation than the simple hydride; was stable, and acted excellently as an anæsthetic. After carefully testing the vapour of this compound, I administered it fourteen times in cases of extraction of teeth, and with results almost identical with those that followed the administration of simple hydramyl.

Finally, in repeating the process of distillation, the first portion that distilled over was set aside, and none was collected until the temperature had reached 90°, the temperature being sustained between that degree and the degree of temperature of the human body (98°). By this means there was obtained a fluid still very agreeable to breathe and extremely rapid in its action. This fluid has the specific gravity of rectified ether—viz., ·725. It boils in the warm hand, and may be considered as containing one part of bichloride of methylene in nine of amyl hydride. I propose to call this fluid "hydramyl-chlor." I administered the vapour of this fluid for the first time for a Surgical operation on July 3. The patient was a young woman, who wished to have a large firm molar tooth extracted. I placed two fluid drachms of the fluid in an inhaler specially constructed for it, and let the patient take the inhaler in her own hand and administer to herself. There was good an-

æsthesia in forty seconds, and ten seconds later Mr. Matthews took out the tooth without causing the least pain. The recovery was complete within the minute, and was neither attended with vomiting nor nausea. Since this case I have continued to administer the same form of vapour in short operations, and, so far, have every reason to be satisfied with the results. I have administered it to children as young as 4 years, and to adults of different ages up to 70, and of both sexes. Mr. Matthews has also administered it a great many times for tooth-extraction at the Marylebone Dispensary with success equally good.

Method of Administration.—The method of administration I adopt for the hydramyl-chlor is very simple. Messrs. Krohne and Seseman have made for me an inhaler of leather, on the plan of Randall's inhaler for bichloride of methylene. Instead, however, of having the inhaler perforated at the lower part with a number of holes, it is closed there, and is furnished with a light valve for the admission of air. There is also an escape-valve at the upper part. Inside the inhaler is a lining of domette, covered with an inner fold of muslin.

In application I pour one to one and a half fluid drachms of the anæsthetic solution upon the folds of muslin, and apply the inhaler thus charged gently to the face, covering the nose and mouth. The patient is allowed to inhale until there is a movement of the eyelids—a quick restless movement or twitch of the lids, without any turning-up or other movement of the ball of the eye. This symptom developed—and it is the first symptom that attracts attention in most cases—the inhalation may cease, presuming the operation to be performed is one of short duration. At this period the patient, however, is not unconscious, and is not insensible to pain. Owing to the insolubility of the vapour in the blood, a short space of time is required for the fluid to diffuse; and so it happens that, in eight or ten seconds after the inhalation has ceased, the patient is in a better state for the operation than he is *instantly* afterwards. To the dentist this occurrence is of importance, because he is not placed in condition of hurry—he can often tell his patient to open the mouth, or give other short directions, and then proceed to extract; and he has no occasion at all to use a gag to keep the mouth open. In every case the phenomenon of prolonged action of the anæsthetic after the inhalation has ceased should be remembered by the administrator, otherwise he may narcotise far more deeply than he intended. It is merely necessary to watch for the first signs of involuntary movement; if the hand of the patient holding the inhaler become unsteady, or if the eyelids begin rapidly to move, it is time to suspend the inhalation—to wait a few seconds—to operate. I have stated the quantity of fluid required to be two drachms; but not half of this is, in

reality, used by the patient. It is necessary to have an excess, because there is considerable and unavoidable loss. For dentists' practice I think the loss may be overcome by having constructed a fixed receiver for holding several ounces of the fluid—a receiver so arranged that, on removing a stopper for the admission of air and exit of vapour, the vapour above the surface of the fluid may be inhaled from a simple mouthpiece. Messrs. Krohne and Seseman are at this time carrying out a design for me of this kind.

So far, in speaking of administration, I have referred chiefly to adults. In the cases of children and old people I make no difference as to the quantity of fluid used in an administration. I am rather guided by symptoms and duration of administration. If a child be allowed to hold the inhaler, I notice that in a few seconds the indications of sleep are usually unmistakable; we have only to remove the inhaler and proceed to the operation.

Hydramyl-Ether.—The composition of what is called compound anæsthetic ether for local anæsthesia has already been given. I should add that a compound ether, which I name *hydramyl-ether*, for both local and general anæsthesia, may be made by adding together in equal portions pure hydramyl and absolute ether. The specific gravity of absolute ether (·720), the vapour density (37), and the boiling-point (92° Fahr.) are all conditions so nearly allied to those of hydramyl, that the two fluids, hydramyl and ether, act in combination practically as if they were one. They differ only in the matter of solubility in water and in blood, the ether being soluble in the blood to the extent of eleven parts in the hundred, the hydramyl being insoluble.

In the production of general anæsthesia, the vapour of hydramyl-ether acts differently to the vapours we have previously discussed. It is slower in its action, it produces some sense of suffocation, it causes a little darkening of the face, pulsation of the vessels of the neck, and a series of disagreeable but not dangerous symptoms, from which the hydramyl-chlor is free. Nevertheless, it is a good general anæsthetic, and, though less agreeable, would be found, in a large number of cases, *safer* to inhale. I have no doubt it will receive favour, especially as it supplies the requirement for both local and general anæsthesia.

Conclusion.—The study of the physiological action of the organic hydrides has afforded me much satisfaction, and many months of, I will hope, useful labour. Whether any of the new applications that have sprung out of the study, and that I have ventured to suggest, will find favour with the Profession of Physic, remains to be seen: I do myself no more

than submit them for their trial. Knowing how sublime is the folly of assuming that whatever a man may offer is good because it is his to offer, I put forward such results as, by reading from nature, I have gathered, without thought of forcing them on my fellows, or anticipating for them a fraction of unproven general experience. If every other practitioner cannot use the results with profit, as well as I can, they are useless. If every other practitioner can use one of them, and no more, with profit, I have done a work—little as it in itself may be—which, by multiplication of hands devoted to it, is sure to be a good work: and this is all the earnest man ever can do, though his ambition devour him, body and soul.—*Med. Times and Gazette, Oct.* 21, 1871, *p.* 490.

89.—THE ORGANIC BROMIDES.

By Dr. Benjamin W. Richardson, F.R.S.

[The physiological action of bromine itself—the element—is definite and well pronounced. Inhalation of its diluted vapour produces a peculiar constricting action in the vessels which supply the secreting surfaces, so that these become dry and painful. After a time there is what may be called a reaction, due probably to a temporary paralysis of the vessels, and then there follows a free excretion of fluid, what the older writers would designate a flux or salivation, attended with some degree of local insensibility.]

Applied directly, in the liquid form, to the body, and especially to a mucous surface, it acts as a direct destructive of tissue, not precisely as a caustic, but as a substance which leads to shrinking and slow death, with still more determinate local insensibility.

In combination with other elements, as with potassium, its direct action is modified, but not removed. Passing through the tissues in a condition of fine distribution, and probably separating from its ally, it exerts on the nervous matter its special sedative influence, causing, if it be carried far enough, its direct paralysing influence over the vessels which govern secretion, and leading to a certain extent to decreased sensibility of the nerves which govern common sensibility.

On the whole, bromine may be considered as a medicine which acts primarily on the sympathetic or organic system of the nervous system, and as a modifier of vascular tension; and this, whether it be applied locally and directly, or generally and indirectly—*i.e.* in combination.

Thus we may rationally administer bromine with any other substance with which it will enter into chemical form of com-

bination; we may trust to the development of its due independent action, without regard to the action of the substance with which it may be combined, and we may be satisfied that it will not materially interfere with the action of the agent with which it has been made to combine.

Bromide of Quinine.—Bromide of quinine is formed by subjecting the alkaloid quinia to hydrobromic acid, or by acting on a salt of the alkaloid with bromide of potassium. The bromide of quinine is soluble, and, mixed with simple syrup, is ready for administration as a medicine. I prefer to employ it as a syrup containing one grain of it in every fluid drachm. The dose of this syrup is from one to four fluid drachms.

Bromide of Morphine.—Bromide of morphine is made by a similar process to that used for making bromide of quinine; morphine or a salt of morphine being substituted for quinine or a quinine salt. This compound also makes up best in the form of a syrup, and the preparation I prescribe contains an eighth of a grain of bromide of morphine in a fluid drachm of simple syrup. The dose of this syrup is from one to four fluid drachms.

Bromide of Strychnine.—Bromide of strychnine is made in the same way as the two last-named preparations; strychnine, or a salt of it, taking the place of quinine or morphine. This, again, I always prescribe as a syrup, one thirty-second of a grain of the bromide being contained in one fluid drachm of the simple syrup. The dose of this syrup is from one to four fluid drachms.

Combinations.—I am in the habit of sometimes combining the preparations named above, in order to suit particular cases of disease. For example, I combine the bromide of quinine and morphine in syrup, so that each fluid drachm of the syrup contains a grain of the salt of quinine with an eighth of a grain of the salt of morphine; or I combine the three salts, so that the fluid drachm of syrup contains a grain of the quinine, an eighth of a grain of the morphine, and a thirty-second of a grain of the strychnine salt. Speaking generally of all these salts, I may state that, in action, the bromide throughout, in so far as its action is indicated, is eliminative and sedative. I am satisfied the bromide of quinine can be administered freely, when quinine itself, or other salts of it, cannot be readily tolerated. I am equally clear that the bromide favours the sedative action of morphia, while it at the same time allays the astringency which morphia induces; and lastly, I am satisfied from experiment that bromide reduces, or rather subdues and prolongs, the action of strychnine or muscular motion.

Notes on Practice.—I have prescribed bromide of quinine, and the other bromides named, in a large number of cases of dis-

ease, and with results I did not fully expect. I will proceed briefly to indicate the leading facts that have occurred to me in the course of observation.

Bromide of quinine simply appears to me to be of good service in cases where certain special and persistent symptoms follow upon syphilis. I hardly speak now of the symptoms which patients themselves connect with that malady, but rather of those insidious symptoms which we, as medical men, who have lived long enough to have seen years of practice, trace back to a syphilitic basis, hereditary or acquired. A case of recurring rheumatism of this nature; a case of recurring ulceration of the fauces; a case of general nervous exhaustion, with flying pains in the limbs, loss of appetite, general debility, loss of hair, and remaining thickening enlargement in the groin, a sequence of bubo: these have been instances in which the administration of the bromide of quinine, in doses of from two to three grains three times a day, has been more immediately and determinately beneficial than any other treatment I have either practised myself, or seen practised by my brethren of physic, in such forms of disease.

One great advantage of this preparation seems to me to be, that it allows one to give much larger doses of quinine than are common, and in frequent and continued doses without setting up the symptoms of headache, oppression, and singing in the ears, which mark what has been called cinchonism. Thus we may give three grains of bromide of quinine, three times a day, without inconvenience for several days if a smaller dose does not suffice.

I have an idea that the bromide of quinine might be administered with advantage in the earlier stages of the contagious diseases, such as small-pox. It would, I think, allay the severe nervous symptoms which usher in these diseases, and so moderate the secondary symptoms that follow in train. Since I began to introduce the bromide into practice, I have not had an opportunity of putting this suggestion to the test, but I have sent some of the preparation to Mr. Marson, of the Small-pox Hospital, asking him to give it impartial trial. I have also asked my friend Dr. Broadbent to make trial of it at the Fever Hospital in cases of acute febrile disorders. The results they obtain I shall hope to communicate in a future number of this journal.

Bromide of morphine is a useful addition to the salts of the alkaloid. It seems to me that a smaller dose of the salt than is effective in the case of the other morphine salts produces as distinct a narcotic influence, and also that the dose may be repeated more frequently without producing those after-effects of an opiate which tell against repetition of administration. For

instance, in a case of extreme depression of a nervous kind, attended with determinate insania, in which, owing to the headache and nausea it produced, the muriate of morphia had been replaced by chloral hydrate, as the latter remedy had been continued until it had become hurtful, I prescribed the fourth of a grain of bromide of morphia at bedtime with excellent results, producing sleep without production of nausea or other distressing symptom. Knowing too well how apt we are to ascribe an efficacy to new remedies which belongs to other causes, I pen these first impressions on the action of this bromide with all due reserve. I write, in fact, mainly to secure the larger experience which will ensue when many acute observers are bringing the same remedy into daily use.

The Bromides of Quinine and Morphine in combination constitute a remedy of which, in cases suited for their administration, I cannot speak too favourably. Four classes of disease seem to me to be specially benefited by this compound; viz., neuralgic fever, cerebral irritation, diabetic phthisis, and extreme acute attacks of intermittent pulse, the result of organic nervous shock. In acute neuralgia I administer a drachm of the syrup of bromide of quinine and morphia to an adult every two hours until the pain is altogether removed, and am able to report not only that pains can be effectually removed by it, but that the medicine exerts no derangement of the body that lessens its value. It calms pain without inducing deep narcotism, it interferes little with the secretions, it rarely causes nausea, and it interferes little with the appetite. In the case of an esteemed member of our own profession, who has been for twelve months under my care, suffering from right hemiplegia, the most distressing symptom I have had to meet has been intense sciatic neuralgia. After a run of all narcotic tonic measures, I found happily in the bromide of quinine and iron a remedy which has now for three months held him free of suffering, and, as a consequence of freedom from pain and sleepless weariness, has led to a distinct improvement in his general health.

In diabetic phthisis I have administered the bromide of quinine and morphia with the same freedom. Under its influence, in these cases, the quantity of sugar and of fluid excreted by the urine notably decreases, cough is relieved, the appetite and digesting power is improved, and recurrent hectic is held in abeyance more certainly, I think, than by any other remedy or combination of remedies with which I am practically conversant.

In a case of intermittent pulse, where the lapse in the heart-stroke was painfully frequent, where there was continued feverish restlessness, and a fear of going to sleep that more

than all sustained the irregular nervous action, the symptoms gave way at once under a few doses of bromide of quinine and morphia in a manner that was as gratifying to the prescriber as to the patient. The purpose of the medicine, in a word, was promptly fulfilled, and as demonstrably as if it had afforded mechanical instead of therapeutical relief. In a second case of intermittent pulse, where the intermittency is the prelude of great mental excitement, followed by depression and melancholia, the remedy has exerted a similar beneficent influence. It induces rest and sleep without the production of deep narcotism and without deranging digestion.

The Bromide of Strychnine has rendered unquestionable service in a few cases of dyspepsia with and from deficient nervous control over the vascular supply of the organs concerned in the process of digestion, in cases of partial organic nervous paralysis of the ventrical division of the organic nervous system. In such cases of disease, and they are by no means uncommon, where, when the body is without food, there is a knowledge of hunger without the true sense of it; when there is congestion of liver, and suppressed secretion to-day, accompanied by giddiness and irritability and præcordial oppression with diarrhœa to-morrow, and then constipation: in these cases the bromine of strychnine in the proportion of one thirty-second of a grain may be given three times daily with marked advantage, an alterative being at the same time occasionally added.

In some mixed cases of nervous pain, with want of organic nervous action in the digestive organs, I have combined the bromide of strychnine with bromide of quinine, and in many cases of this nature I have prescribed the three bromides with good result.

Syrup of the bromide of quinine and strychnine, and syrup of the bromide of quinine, morphine, and strychnine, will both, I believe, become favourite compounds with the profession, finding their place as Eastin's syrup of the superphosphate of iron, quinine, and strychnine has found its place in the list of tried and approved medicaments.

One other point of practice remains to me only to note. In cases where there is much dryness and irritability of the mucous membrane of the pharynx and larynx, the bromides are not commendable; the bromine increases the irritation. This was so marked in a case where there was a small ulcerated surface in the larynx, that I had to stop the administration altogether, the smallest dose producing violent and long-continued irritative cough and spasm.—*Practitioner, June,* 1871, *p.* 337.

90.—CHLORAL HYDRATE: ITS USE IN LUNACY PRACTICE ILLUSTRATED.

By Dr. N. G. MERCER, Senior Assistant Medical Officer County Lunatic Asylum, Lancaster.

The addition of an hypnotic agent of high value to the resources of our Materia Medica is obviously of the first importance in the medical practice of asylums. Apart from the mere curative power exerted by drugs which are administered with the view and effect of producing sleep, it soon becomes apparent, in an intercourse from year to year with the society of a large asylum, that the comforts of the inhabitants are promoted in a wonderful degree by making their noisy neighbours of peaceful habits, especially during the night. It will scarcely be considered a slight upon some highly respectable soporific medicines, which have been erewhile recommended and employed from time to time, if we summarise the results of experience by saying that, when contrasted with opium and its preparations, their utility dwindles into comparative insignificance. It would be out of place here to dilate at length on the precious service rendered by opiates in the many varieties of mental infirmity. Let me, however, make one observation, which must be familiar to all asylum practitioners,—viz., how great is the change for the better produced in the general comfort of a ward where a patient of particularly, or continuously noisy habits, lives, by the administration to that patient of morphia at intervals. Under its influence clamorous excitement subsides; a disposition to quarrel is for a time completely subdued; obscenity, execrations, and abuse, no longer constantly offend the ears of convalescent and of sensitive neighbours; and it is well understood that a temporary tranquillity of mind, in a person with such troublesome tendencies, is attended (for the quieter inmates of the ward) with a transition into a state of comparative Elysium. These remarks especially apply to the female wards, and it is improbable that any remedy will be found to supersede opium in achieving such results. Unfortunately, however, the sedative effects of the drug becomes less and less marked by repetition of the dose, and it requires to be increased before they are developed; ultimately the medicine is for a time abandoned, so that its action may possess increased impulse when resumed. The inconvenience felt from first having to enlarge to a great extent the dose of opium, and, again, from having to intermit its administration on account of the negative results, has been especially experienced when a full hypnotic action has been sought, and the medicine is given in the form of a draught at bed-time. This leads me to observe that, used with this object, the chloral

hydrate appears to possess certain advantages. Thus, suppose thirty grains of the latter medicine be given in a draught to a restless and sleepless patient, and a good night's sleep follow its exhibition, we shall find that the drug does not lose its potency by repetition; as, after the lapse of a few weeks, during which time it has been regularly taken, the same quantity of dose will produce effects as marked and as happy as when first administered. Although prone to view with suspicion any medicine claiming to rival opium, I have been compelled, by experience of the virtues of chloral hydrate in practice, to attribute to it this advantage and superiority over any opiate as regards the effects of an uniform dose.

[Dr. Mercer then relates eleven cases, and concludes as follows:]

From a study of these cases the following conclusions on the virtues of chloral hydrate given as an hypnotic draught seem fairly deducible:—1. In very many bad cases of noisy habits during night it is a powerful and efficient hypnotic, and more certain than an opiate. 2. The effects of the drug do not fail of development through long continued use of it in an uniform dose. 3. In the most intractable cases of noisy habits it is probably of less service than an opiate—such cases, it would seem, as those of intensely maniacal and excited patients who in a previous attack of mental derangement presented the symptoms of melancholia.

Given thus, then, as an hypnotic draught, the cholral hydrate appears entitled to a high position in the future of lunacy practice. As the progress of recovery proceeds apace with the establishment of sound sleep, it is obvious how, as a therapeutic agent, its effects must be in a very large degree curative and remedial. This remark applies to attacks of acute disease. But surely, in the chronic insane, that mental agony produced by the constantly-returning presence of delusive voices and visions in the long nights of maniacal clamour and excitement is an object scarcely less worthy of attack and of relief than is the sensation of pain in those who are of sound mind. The collateral happy results of this artificial rest are not to be measured except by those who are themselves the nervous, timid, and excitable neighbours of patients whose constant habit it has been to "make night hideous" by their cries.

The eleven cases recorded here are selected as being some of the earliest which were used to test the efficacy of chloral hydrate in this Asylum. But it has been largely employed in a great number of other cases, including some of acute mania, of hysteria attended with suicidal impulses, and of general paralysis accompanied, as it so often is, with dangerous and distressing restlessness. It is an anxious matter to remove a

suicidal patient from a dormitory to a single room, because in the latter, with all the means at our disposal, a patient bent upon self-destruction might probably concoct a plan to compass the ghastly purpose. A medicine which will enable such a patient to pass tranquil nights among watchful neighbours in an associated room is one whose precious service cannot be over-estimated. I have not been able to verify the observation, put forth upon respectable authority, that the hypnotic effects of chloral hydrate are sometimes postponed for twenty-four hours; that a draught given to-night, for example, will not affect the patient's sleeplessness to-night, but will cause it to vanish to-morrow night, without repetition of the draught. Should such apparent connexion between supposed cause and effect arise, it seems to me it would be very difficult to establish its reality unassisted by imagination.

It only remains for me to add that any attempts which I have made to substitute chloral hydrate for opium as a partial sedative by day have been attended with negative results. I give up using it for this purpose, as morphia was found far more useful. Thus, to a restless, meddling, and noisy general paralytic, aged 30—puerperal case—who is in the third stage of her disease, twenty-five grains of chloral hydrate were given at intervals, but her excitement was not subdued until she had full doses of morphia. The chloral has made some patients sleep in the day-time, but has produced no tranquillising influence during their waking hours.—*Medical Times and Gazette, April* 22, 1871, *p.* 450.

91.—CHLORAL IN TOOTHACHE.

By Dr. David Page, Kirkby Lonsdale.

For some time past I have been in the habit of using chloral hydrate, not only as an internal sedative in cases of severe dental neuralgia and of caries where it was at the time inadvisable to permit extraction, but more frequently as a direct local application to the carious tooth. A few grains of the solid hydrate, introduced into the cavity of the tooth upon the point of a quill, speedily dissolve there; and, in the course of a few minutes, during which a not unpleasant warm sensation is experienced, the pain is either deadened, or more often effectually allayed. A second or third application may be resorted to if necessary. The hours of agony that various circumstances may otherwise entail are in this way avoided; and, in the last instance under my notice, a young lady in a delicate state of health, suffering intensely from a carious tooth, received immediate relief from this application. She lived

at a great distance from a dentist, and was unwilling to have the tooth extracted without some anæsthetic; and the chloral hydrate was the means of obtaining a sound night's rest. I find in my notes her own report that "the toothache was relieved in a few minutes; aching did not return all night; and when it did in the morning, was again stopped by the same means.—*British Medical Journal, Sept.* 2, 1871, *p.* 262.

92.—DIAGNOSIS OF STRICTURE OF THE RECTUM BY THE SEX.

[The following is taken from a report of Mr. Hutchinson's clinique.]

A case of stricture of the rectum called forth the remark that, speaking roughly, it was sufficient to ask the sex of the patient to ascertain the nature of the stricture, whether malignant or innocent; malignant stricture being almost limited to men, whilst in women the disease was as frequently innocent, syphilitic, gonorrhœal, or following parturition.—*Medical Times and Gazette, July* 1, 1871, *p.* 8.

93.—VULCANITE URINOMETERS.

At the suggestion of Mr. Pollock, Messrs. Blaise, of St. James's Street, have turned their attention to the adaptation of vulcanite to the manufacture of urinometers, hydrometers, alcoholimeters, saccharometers, and other instruments for indicating the density of liquids; and they have found, it seems, after repeated trials and tests, that it is better suited to the construction of the above-named instruments than either glass or metal.

Vulcanite is perfectly proof against all kinds of acid, except bisulphide of carbon. Instruments made with it, therefore, do not get out of order or break, if used with ordinary care; whereas slight taps will often break those made of glass, or a fall will injure those made of metal, which are likewise apt to corrode. The vulcanite instrument takes its gravity much quicker than one made of glass or metal, and is cheaper and more durable than either.

We think the attention of the profession may well be directed to the different instruments prepared in vulcanite.—*Lancet, Oct.* 28, 1871, *p.* 612.

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